PROCEEDINGS

OF

THE ROYAL SOCIETY.

1849. No. 75.

Dec. 6, 1849.

GEORGE RENNIE, Esq., Vice-President, in the Chair.

The Chairman announced that the Earl of Rosse had nominated as Vice-Presidents:—

George Rennie, Esq.
Sir B. C. Brodie, Bart.
Sir Roderick I. Murchison.
Professor Owen.
Lieut.-Col. Reid, R.E.
Peter Mark Roget, M.D.

The Right Hon. Thomas Babington Macaulay was admitted into the Society.

Dec. 13, 1849.

In consequence of the funeral of Her late Majesty the Queen Dowager taking place this day, no Meeting of the Society was held.

December 20, 1849.

Dr. ROGET, Vice-President, in the Chair.

A. C. Ramsay, Esq., was admitted into the Society.

The Bakerian Lecture was delivered by Professor Graham, F.R.S., "On the Diffusion of Liquids."

The apparatus used in studying the diffusion of salts and other substances into water was very simple. It consisted of an open phial to contain the solution of the salt to be diffused, which was entirely immersed in a large jar of pure water, so that the solution in the phial communicated freely with the latter. Phials cast in a mould of the capacity of four ounces of water, or more nearly 2000 grains, were generally employed, which were ground down to a uniform height of 3·8 inches. The neck was 0·5 inch in depth, and the

aperture or mouth of the phial 1·25 inch in diameter. The phial was filled up with the solution to be diffused till it reached the point of a pin dipping exactly 0·5 inch into the mouth of the bottle. This being the solution cell or bottle, and the external jar the "water-jar," the pair together form a "diffusion cell." The diffusion was stopped, generally after seven or eight days, by closing the mouth of the phial with a plate of glass, and then raising it out of the water-jar. The quantity of salt which had found its way into the water-jar—the diffusion product as it was called—was then determined by evaporating to dryness.

The characters of liquid diffusion were first examined in detail with reference to common salt.

It was found, first, that with solutions containing 1, 2, 3 and 4 per cent. of salt, the quantities which diffused out of the phials into the water of the jars, and were obtained by evaporating the latter, in a constant period of eight days, were as nearly in proportion to these numbers, as 1, 1·99, 3·01 and 4·00; and that in repetitions of the experiments, the results did not vary more than 1-40th part. The proportion of salt which diffused out in such experiments amounted to about 1-8th of the whole.

Secondly, that the proportion of salt diffused increases with the temperature; an elevation of 80° Fahr. doubling the quantity of chloride of sodium diffused in the same time.

The diffusibility of a variety of substances was next compared, a solution of 20 parts of the substance in 100 water being always used. Some of the results were as follows, the quantities diffused being expressed in grains: chloride of sodium 58·68, sulphate of magnesia 27·42, sulphate of water 69·32, crystallized cane-sugar 26·74, starch-sugar 26·94, gum-arabic 13·24, albumen 3·03. The low diffusibility of albumen is very remarkable, and the value of this property in retaining the serous fluids within the blood-vessels at once suggests itself. It was further observed, that common salt, sugar and urea, added to the albumen under diffusion, diffused away from the latter as readily as from their aqueous solutions. Urea itself is as highly diffusible as chloride of sodium.

In comparing the diffusion of salts dissolved in 10 times their weight of water, it was found that isomorphous compounds generally had an equal diffusibility, chloride of potassium corresponding with chloride of ammonium, nitrate of potash with nitrate of ammonia, and sulphate of magnesia with sulphate of zinc. The most remarkable circumstance is that these pairs are "equi-diffusive," not for chemically equivalent quantities, but for equal weights simply. The acids differed greatly in diffusibility, nitric acid being nearly four times more diffusive than phosphoric acid; but these substances also fell into groups, nitric and hydrochloric acids appearing to be equally diffusive; so also acetic and sulphuric acids. Soluble sub-salts and the ammoniated salts of the metals present a surprisingly low diffusibility; the quantities diffused, under similar circumstances, of the three salts, sulphate of ammonia, sulphate of copper, and the blue ammonio-sulphate of copper being very nearly as 8, 4 and 1.

When two salts are mixed in the solution-cell, they diffuse out into the water atmosphere separately and independently of each other, according to their individual diffusibilities. This is quite analogous to what happens when mixed gases are diffused into air. An important consequence is, that in liquid diffusion we have a new method of separation or analysis for many soluble bodies, quite analogous in principle to the separation of unequally volatile substances in the process of distillation. Thus, it was shown that chlorides diffuse out from sulphates and carbonates, and salts of potash from salts of soda; and that from sea-water the salts of soda diffuse out into pure water faster than the salts of magnesia. The latter circumstance was applied to explain the discordant results which have been obtained by different chemists in the analyses of the water of the Dead Sea, taken near the surface; the different salts diffusing up into the sheet of fresh water, with which the lake is periodically covered, with unequal velocity.

It was further shown that chemical decompositions may be produced by liquid diffusion; the constituents of a double salt of so much stability as common alum being separated, and the sulphate of potash diffusing in the largest proportion. In fact, the diffusive force is one of great energy, and quite as capable of breaking up compounds as the unequal volatility of their constituents. Many empirical operations in the chemical arts, it was said, have their foundation in such decompositions.

Again, one salt, such as nitrate of potash, will diffuse into a solution of another salt, such as nitrate of ammonia, as rapidly as into pure water; the salts appearing mutually diffusible, as gases are known to be.

Lastly, the diffusibilities of the salts into water, like those of the gases into air, appear to be connected by simple numerical relations. These relations are best observed when dilute solutions of the salts are diffused from the solution-cell, such as 4, 2 or even 1 per cent. of salt. The quantities diffused in the same time from 4 per cent. solutions of the three salts, carbonate of potash, sulphate of potash and sulphate of ammonia, were 10·25, 10·57 and 10·51 grains respectively; and a similar approach to equality was observed in the 1, 2, and 6$\frac{2}{3}$ per cent. solutions of the same salts. It also held at different temperatures. The acetate of potash appeared to coincide in diffusibility with the same group, and so did the ferrocyanide of potassium. The nitrate of potash, chlorate of potash, nitrate of ammonia, chloride of potassium and chloride of ammonium formed another equi-diffusive group. The *times* in which an equal amount of diffusion took place in these two groups appear to be as 1 for the second to 1·4142 for the first, or as 1 to the square root of 2. Now in gases, *the squares of the times* of equal diffusion are *the densities of the gases.* The relation between the sulphate of potash and nitrate of potash groups would therefore fall, to be referred to the diffusion molecule or diffusion vapour of the first group having a density represented by 2, while that of the second group is represented by 1.

The corresponding salts of soda appeared to fall into a nitrate and

sulphate group also, which have the same relation to each other as the potash salts.

The relation of the salts of potash to those of soda, in times of equal diffusibility, appeared to be as the square root of 2 to the square root of 3; which gives the relation in density of their diffusion molecules, as 2 to 3. Hydrate of potash and sulphate of magnesia were less fully examined, but the first presented sensibly double the diffusibility of sulphate of potash, and four times the diffusibility of the sulphate of magnesia. If these times are all squared, the following remarkable ratios are obtained for the densities of the diffusion molecules of these different salts, each of which is the type of a class of salts, hydrate of potash 1, nitrate of potash 2, sulphate of potash 4, sulphate of magnesia 16, with nitrate of soda 3 and sulphate of soda 6.

In conclusion, it was observed, that it is these diffusion molecules of the salts which are concerned in solubility, and not the Daltonian atoms or equivalents of chemical combination; and the application was indicated of the knowledge of the diffusibilities of different substances to a proper study of endosmose.

January 10, 1850.

GEORGE RENNIE, Esq., Vice-President, Treasurer, in the Chair.

The Right Rev. The Lord Bishop of Manchester was admitted into the Society.

The following papers were read:—

1. "Experiments and Observations upon the Properties of Light." By Lord Brougham, F.R.S. &c.

The author states that the optical inquiries of which he here gives an account were conducted in the first instance under the most favourable circumstances, arising from the climate of Provence, where they were commenced, being peculiarly adapted to such studies: he further states that he subsequently had the great benefit of a most excellent set of instruments made by M. Soleil of Paris; remarking, however, that this delicate apparatus is only required for experiments of a kind to depend upon nice measurements, and that all the principles which he has to note in this paper as the result of his experiments can be made with the most simple apparatus and without any difficulty or expense. His statement of the results of his experiments is thrown into the form of definitions and propositions, for the purpose of making it shorter and more distinct, and of subjecting his doctrines to a fuller scrutiny. He premises that he purposely avoids all arguments and suggestions upon the two rival theories, the Newtonian or Atomic, and the Undulatory.

The following are the author's Definitions and Propositions.

Definitions.

1. *Flexion* is the bending of the rays of light out of their course in passing near bodies.

2. Flexion is of two kinds—*inflexion*, or the bending towards the body; *deflexion*, or the bending from the body.
3. *Flexibility, deflexibility, inflexibility* express the disposition of the homogeneous or colour-making rays to be bent, deflected, inflected by bodies near which they pass.

Proposition I.

The flexion of any pencil or beam, whether of white or of homogeneous light, is in some constant proportion to the breadth of the coloured fringes formed by the rays after passing by the bending body. Those fringes are not three, but a very great number, continually decreasing as they recede from the bending body, in deflexion, where only one bending body is acting; and they are real images of the luminous body by whose light they are formed.

Proposition II.

The rays of light when inflected by bodies near which they pass are thrown into a condition or state which disposes them to be on one side more easily deflected than they were before the first flexion; and disposes them on the other side to be less easily deflected: and when deflected by bodies they are thrown into a condition or state which disposes them to be more easily inflected, and on the other side to be less easily inflected than they were before the first flexion.

Proposition III.

The disposition communicated to the rays by the flexion is alternative; and after inflexion they cannot be again inflected on either side; nor after deflexion can they be deflected. But they may be deflected after inflexion, and inflected after deflexion, by acting on the sides disposed, and not by acting upon the sides polarized.

Proposition IV.

The disposition impressed upon the rays, whether to be easily deflected or easily inflected, is strongest nearest the first bending body, and decreases as the distance increases.

Proposition V.

The fringes made by the second body acting upon the rays deflected by the first, must, according to the calculus applied to the case, be broader than those made by the second body deflecting those rays inflected by the first.

Proposition VI.

When one body only acts upon the rays, it must, by deflexion, form them into fringes or images decreasing as the distance from the bending body increases. But when the rays deflected and disposed by one body are afterwards inflected by a second body, the fringes will increase as they recede from the direct rays. Also when the fringes made by the inflexion of one body, and which increase with the distance from the direct rays, are deflected by a second body,

the effect of the disposition and of the distances is such as to correct the effect of the first flexion, and the fringes by deflexion of the second body are made to decrease as they recede from the direct rays.

Proposition VII.

It is proved by experiment that the inflexion of the second body makes broader fringes or images than its deflexion, after the deflexion and inflexion of the first body respectively; and also that the deflexion fringes decrease, and the inflexion fringes increase with the distance from the direct rays.

Proposition VIII.

The joint action of two bodies situated similarly with respect to the rays which pass between them so near as to be affected by both bodies, must, whatever be the law of their action, provided it be inversely as some power of the distance, produce fringes or images which increase with the distance from the direct rays.

Proposition IX.

It is proved by experiment that the fringes or images increase as the distance increases from the direct rays.

These propositions are illustrated by particular instances, and their truth is shown by experiments and by some mathematical investigations. The author concludes his paper by a few observations tending further to illustrate and confirm the foregoing propositions, and for the purpose of removing one or two difficulties which had occurred to others until they were met by facts, and also of showing the tendency of the results at which he had arrived.

2. "Electro-Physiological Researches."—7th Series. By Signor C. Matteucci. Communicated by W. R. Grove, Esq., F.R.S.

In this memoir, Prof. Matteucci, after recapitulating the results of his previous researches on electro-physiology, published in the Philosophical Transactions, proceeds to the relation of new experiments. He first shows that nervous filaments made to conduct an electric current in a liquid are not capable, like metallic wires, of acting as electroids, and giving rise to electro-chemical decomposition. The solution employed was that of iodide of potassium; the nerves, two large ones taken from a living animal, each of which was separately attached to the metallic extremities of a pile of fifteen couples. No trace of decomposition followed; and he concludes from hence, that the conductibility of nervous matter is due to the liquid part of the matter itself.

He then gives further experiments on the relative conductibility of muscles and nerves, with a view to ascertain whether, when a current was impelled through a mass of muscle, any part of the current might have passed through the nervous filaments spread through that muscle. For this purpose he inserted the nerve of a galvanoscopic frog into a hole made in a piece of dead muscle, through which he then passed a very powerful current: no contraction followed

in the galvanoscopic frog. When muscles still retaining their irritability were substituted for the dead muscle, induced contractions occurred in the galvanoscopic frog during the passage of the current. He concludes that when the poles of a pile of twenty-five or thirty elements are applied to the surface of the muscles of a living animal, the phenomena produced by the passage of the current must depend either on the *direct* action of the current on the muscular fibre, or on the *indirect* action or *influence* of the electric current transmitted by the muscular fibre to its own nervous filaments, or rather to the nervous force existing in those filaments.

Referring then to an experiment related in a preceding paper, in which the lower limbs of a frog, united to the spine only by the lumbar nerves, are placed astride two glasses containing water, with each foot immersed, and in which a current, after traversing the two limbs, and consequently the two nerves, in opposite directions, so modifies at length the excitability of the nerves, that, on opening the circuit, only the limb in which the current has been passing inversely contracts, he shows that if in this state what may be called the 'inverse' nerve be touched by a piece of muscle, although the circuit is continued, yet the limb contracts as though the circuit had been broken. In fact, the muscle, by its greater conductibility, becomes traversed by the current in place of the nerve. Again, if after the former part of the experiment has been performed, the portions of nerve which had hitherto been buried among the crural muscles be dissected out, it is easily seen that their excitability has not been affected like that of the lumbar nerves, because the current in place of traversing them has traversed only the crural muscles. The nerve has had its excitability modified in only that part of its course in which, being laid bare and isolated, it has necessarily conducted the current.

M. Du Bois Reymond (*Comptes Rendus*) has related an experiment seeming to lead to the inference that section of the spinal marrow increases the excitability of the lumbar nerves, at least during a certain period of time. In order to test the accuracy of this conclusion on so important a point, M. Matteucci institutes a number of very accurate experiments, in which he measures the excitability of the lumbar nerves after section of the spinal marrow, by means of the apparatus of Breguet, used and described by him in a former paper. His first results show that "the contraction excited in the muscles of a frog, of which the spinal marrow has been divided from twelve to eighteen hours, is *stronger* than that obtained under the same circumstances from the muscles of a frog just killed, without having been previously subjected to any injury to its nervous system." But subsequent experiments have satisfied him that this result depends not on the separation from the spinal marrow, but rather on the repose in which the muscle has been permitted to remain; for without division of the marrow, nearly the same force of contraction existed after the same interval of time. He finds indeed that the only alteration which the excitability of a nerve undergoes by separation from the nervous centres, consists in its being more readily

exhausted under the action of stimulants, the longer the period that has elapsed since its detachment.

The author then proceeds to relate the nature of the strict analogy existing between electricity and nervous force. As electricity is developed under the influence of the nervous current in the organs of electrical fishes, so, as a converse of this phenomenon, electricity may develope the nervous force. After adverting to the well-known analogy subsisting in every particular between the phenomena of the electrical organ and those of muscles, he adverts to the old experiment of passing a current through the muscles of the thighs of a living animal, the positive pole being placed now above, now below, so that it may be supposed that the current passes in the two cases in opposite directions as regards the nervous filaments distributed in the muscles. He then points out that the effects of a current directed downwards, in the direct course of the nerves, are a strong contraction of the muscle traversed, and also of the *muscles of the leg below*; while the effect of a current in the opposite, or inverse direction, is *pain*, together with contractions less violent and always confined to the muscles traversed. The *contractions* (especially of the parts below) indicate a current of nervous force propagated towards the muscles, while the *pain* indicates a current towards the nervous centre. Now, bearing in mind that it has been proved by direct experiments that an electric current traversing a muscle never quits the muscular fibre to enter the nervous filaments, it seems clear that the phenomena just spoken of are exclusively owing to the *influence* exerted by the electricity passing through the muscles on the nervous force contained in the nerves; and also that this nervous force acts peripherad or centrad according to the direction of the electric current which excites it. The great importance of the conclusions drawn from these experiments consists in this, that they lead to the same law which establishes the analogy between nervous force and the electrical discharge of fishes. The paper concludes with some further considerations intended to confirm this law.

January 17, 1850.

SIR RODERICK I. MURCHISON, Vice-President, in the Chair.

A paper was read, entitled "Researches respecting the Molecular constitution of the Volatile Organic Bases." By Dr. A. W. Hofmann. Communicated by Sir James Clark, Bart., F.R.S.

Chemists, although all acknowledging the existence of an intimate relation between the vegetable alkaloids and ammonia, are nevertheless divided in their opinions respecting the nature of this connection, two theories having been propounded upon the subject. According to the one, that of Berzelius, the bases would have to be considered as conjugated ammonias in which ammonia still pre-exists as such; while according to Liebig's views, these substances are represented as

amides, *i.e.* as ammonia in which one equivalent of hydrogen is eliminated and replaced by an equivalent of a compound radical.

The researches of the author prove that the theory of Berzelius is inadmissible, at all events for the volatile organic bases, inasmuch as in these substances ammonia ceases to exist as such. They show, moreover, that Liebig's view, although correctly expressing the constitution of by far the greater number of the volatile bases known, and presenting, when considered at the time it was first propounded, a wonderful anticipation of subsequent discovery, represents nevertheless only a special case of a much more general relation. The result at which the author has arrived is, *that ammonia is capable of losing either* 1 (*Liebig's case*) *or* 2 *or* 3 *equivs. of hydrogen which are respectively replaced by* 1, 2 *or* 3 *equivs. of the same, or various compound radicals, a variety of substances apparently endless being produced, in which its fundamental property* (*the basic character*) *is retained, although modified by the number of radicals introduced and their position in the scale of organic compounds.*

In support of this statement, he adduces the artificial construction of thirteen new organic alkaloids, formed by a method which affords the means of increasing the number of these substances to an indefinite extent. This method consists in exposing ammonia to the action of the chlorides, bromides or iodides of the alcohol radicals, which remove 1 equivalent of hydrogen of the latter, as hydrochloric, hydrobromic, &c. acid, while the remaining constituents, assuming the alcohol-radical, give rise to the formation of an organic base which unites with the hydrogen acid.

By subjecting the new base itself to a similar treatment, another equivalent of the two remaining equivalents of hydrogen may be removed, a second organic base being formed, which in its turn gives rise to a third.

The changes which the ammonia undergoes in these various processes may be represented graphically by the following simple formulæ, X, Y and Z, denoting generally compound radicals.

$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N+XBr=\left.\begin{matrix}H\\H\\X\end{matrix}\right\}N.HBr.$$

$$\left.\begin{matrix}H\\H\\X\end{matrix}\right\}N+YBr=\left.\begin{matrix}H\\X\\Y\end{matrix}\right\}N.HBr.$$

$$\left.\begin{matrix}H\\X\\Y\end{matrix}\right\}N+ZBr=\left.\begin{matrix}X\\Y\\Z\end{matrix}\right\}N.HBr.$$

For the illustration of these general formulæ, one of the numerous sets of experiments which the author has communicated in his paper may be quoted in which $X=Y=Z$. Ammonia, when exposed to the action of bromide of ethyl (hydrobromic ether), is converted into hydrobromate of ethylamine, *i.e.* ammonia in which 1 equivalent of hydrogen is replaced by ethyl, a compound which was first observed by M. Wurtz under perfectly different circumstances. Ethylamine, treated again with bromide of ethyl, yields a new alkaloid diethyla-

mine, *i.e.* ammonia in which 2 equivalents of hydrogen are replaced by ethyl, and which, under the influence of a further quantity of bromide of ethyl, lastly is transformed into triethylamine, or ammonia in which the whole of the hydrogen is replaced by ethyl. This is a most powerful alkali, whose properties resemble those of caustic potassa.

January 24, 1850.

RICHARD OWEN, Esq., Vice-President, in the Chair.

The following communications were read:

1. "Observations on the Freezing of the Albumen of Eggs." By James Paget, Esq., Professor of Anatomy and Surgery to the Royal College of Surgeons. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

The object of this paper is to illustrate a peculiar property of the albumen of the eggs of birds, a property which seems to have its purpose in preserving them from the injurious effects of very low temperatures.

Mr. Hunter observed that a fresh egg will resist freezing longer than one which has been previously frozen and thawed; and he referred this fact to the 'vital power' of the egg in the first case, and the destruction of that power by freezing in the second. The author's experiments confirm those of Mr. Hunter, and prove, also, that when fresh eggs are exposed to very low temperatures, and also in the case of eggs which are decayed, or putrid, or the contents of which have been much altered by mechanical force or by electricity, a shorter time is sufficient for the freezing of such eggs, than is necessary for the freezing of those which are uninjured.

An examination of the rates at which heat was lost by the several eggs, exposed to temperatures varying from zero to 10° Fahr., showed that fresh eggs, though they resist freezing longer than any others, yet lose heat more quickly; and that their resistance to freezing is due to the peculiar property of their albumen, the temperature of which may be reduced to 16° Fahr., or much lower without freezing, although its proper freezing-point is at or just below 32°. Other than fresh eggs lose heat comparatively slowly, but freeze as soon as their temperature is reduced to 32°; fresh eggs lose heat more quickly, but may be reduced to 16° or lower; then, at the instant of beginning to freeze, their temperature rises to 32°.

That this peculiarity of fresh eggs is not due to vital properties, is proved by experiments which show that certain injuries, such as mechanical violence, addition of water, and others, which spoil their powers of resisting freezing, do not prevent eggs from being developed in incubation. By the same and other experiments, which are related, it is made probable that the peculiarity depends on the mechanical properties of the albumen; for, whatever makes the albumen more liquid than it is naturally in the fresh egg, destroys the power of resisting freezing.

The author could find no other substance possessing this property; and in evidence of its adaptation to the purpose of preserving eggs from the loss of their capacity of developement, which they would suffer in being frozen, he relates experiments in which eggs were kept for a considerable time at temperatures ranging from zero to 10° Fahr., yet were afterwards developed in incubation. By the same series of experiments it was shown, that, although freezing renders the effectual developement of the germ impossible, yet the intensest cold, if freezing does not take place, has no similar result.

2. A Letter from M. Kupffer, to Lieut.-Col. Sabine, For. Sec. R.S., "On the establishment of a Central Physical Observatory at St. Petersburg." Communicated by Lieut.-Col. Sabine.

Observatoire Physique Central, St. Pétersbourg,
ce 21 Juillet, 2 Août, 1849.

Monsieur et cher ami,—Je suis heureux de pouvoir vous annoncer que l'observatoire physique central, dont le projet a été confirmé par S. M. l'Empereur, il y a deux ans, est achevé, et son activité a commencé dès le 1er Juillet.

Cet établissement est destiné, comme j'ai déjà eu le plaisir de vous écrire il y a longtemps, à former un point central pour nos observatoires magnétiques et météorologiques, et en général pour tout ce qui se fait, dans l'étendue de l'Empire de Russie, pour la météorologie et le magnétisme terrestre; il y a un emplacement suffisamment grand pour contenir plusieurs salles d'observation, pour loger le directeur et son secrétaire et plusieurs calculateurs, pour une bibliothèque et l'archive des observations magnétiques et météorologiques envoyées de differens points de l'Empire. Le directeur de l'observatoire physique central exerce une surveillance active sur toutes les stations magnétiques et météorologiques de l'Empire; il fait de temps en temps des voyages d'inspection; les observateurs qu'on emploie dans ces stations reçoivent leurs instructions de l'observatoire de physique; les instrumens qu'on leur fournit sont vérifiés et comparés aux instrumens de l'observatoire; des instrumens également vérifiés et comparés sont delivrés à tous les physiciens faisant partie d'expéditions ordonnés par le gouvernement; toutes les observations magnétiques et météorologiques qu'on fait dans l'étendue de l'Empire sont adressées au directeur de l'observatoire, appréciées, calculées et imprimées par ses soins: on essaie à l'observatoire central toutes les nouvelles méthodes d'observation avant de les mettre en pratique, on s'y occupe de leur perfectionnement; enfin on y trouve réunis tous les moyens nécessaires pour faire des recherches dans toutes les branches de la physique qui sont dans un rapport plus ou moins direct avec la physique de la terre, et qui exigent une exactitude difficile ou impossible à atteindre dans les cabinets de physique ordinaires.

Maintenant je n'ai qu'un seul vœu à former, c'est que l'exemple de notre gouvernement soit bientôt suivi par le gouvernement anglais, qui a déjà fait de si grands sacrifices pour le magnétisme terrestre. Une longue expérience a dû vous apprendre, comme elle me l'a appris, qu'un tel établissement est une nécessité, lorsqu'on

a une si vaste entreprise à diriger que la nôtre, et lorsqu'il s'agit de tirer au clair tous les résultats qu'elle promet. Combien ne se perd-il pas d'argent, de temps et de peines, par des efforts isolés, qui coordonnés convenablement et dirigés vers un but commun porteraient les plus beaux fruits? Combien de données précieuses pour la science ne voient jamais le jour, faute de moyens de les publier?

Je n'hésite pas de dire, que la fondation d'un observatoire physique central en Angleterre me parait être d'une grande nécessité dans l'état actuel des sciences d'observation, et que l'exemple donné par le gouvernement anglais et par le notre serait bientôt suivi par les autres états.

Veuiller agréer les assurances de ma haute considération et de ma vive amitié. (Signée) A. T. Kupffer,

Directeur de l'Observatoire physique central.

3. A Letter from Captain C. M. Elliot, Madras Engineers, to Lieut.-Col. Sabine, For. Sec. R.S., transmitted through the Court of Directors of the East India Company. Communicated by Lieut.-Col. Sabine.

Having undertaken the magnetic survey of the Indian Archipelago at the recommendation of the Royal Society, I think a slight sketch, detailed as briefly as possible, of my operations may not be uninteresting to Sir John Herschel and the Committee of Physics of which he is Chairman, prior to the publication of the Survey. I trust likewise I have acted strictly in accordance with the wishes of those who so kindly recommended me for the Survey, and I hope that my earnest efforts to do my duty will gain for me that approbation which I have under no ordinary difficulties incessantly striven to obtain.

I will in the first place mention the different stations I visited, and then describe in a few words, the way in which the observations were taken.

I have made a most complete survey of Java. At Batavia I established an observatory where observations, magnetic and meteorological, were taken hourly from 3 A.M. to 9 P.M. for nine months. In addition, about fifty stations, where observations of dip, of total intensity, of latitude, longitude, and declination were taken; these were always made by myself, and I am certain they can be depended upon.

In Borneo an observatory was established at Sarawak, where observations were taken quarter-hourly for three months, besides visiting the Dutch settlements of Sambas, Pantianak and Succadana on the western coast.

In Sumatra four months of observation at Padang, besides a magnetic survey comprising about thirty stations. I crossed the equator here as well as at Pantianak in Borneo.

At Singapore I compared the portable instruments with the fixed instruments of the observatory, besides determining the horizontal intensity and dip, which had not been accurately determined previously from insufficiency of means.

At the Cocos or Keeling Islands, six weeks of observation.
At Sambooangan, in the Island of Mindanao, upwards of a week.
At Keemah in Celebes, the same.
At Penang, the same.
At Moulmein, the same.
At the Nicobars, the same.
At Bencoolen in Sumatra, the same.

I will now mention the instruments and the mode of observation at the observatories. The following instruments were registered every hour from 3 A.M. to 9 P.M. Two declinometers, and latterly a third made by Jones; a bifilar magnetometer and its thermometer; a standard barometer and its thermometer by Newman; a standard thermometer and a dry and wet bulb.

On the survey, I employed for the observations four dipping-needles; a portable declinometer with altitude and azimuth instrument for the declination and for latitude; a sextant, artificial horizon and chronometer for the error and rates of the watch, which was but a poor one.

I began work generally at 6 A.M., put up the portable declinometer, and allowed the brass weight and stirrup to swing for a couple of hours thoroughly to take the torsion out of the thread, adjusting it from time to time so that the stirrup might ultimately take up a position in the magnetic meridian; during this period I took the dip. At 8 A.M. I took sights with the sextant, and putting in the collimator magnet, I adjusted the altitude and azimuth to it, and took altitudes and azimuths of the sun, three on the limb direct, and three on the limb reversed, noting the reading on the horizontal limb; at the same time this gave me the reading of the true meridian; the magnetic axis of the collimator magnet was then read off; these observations were usually completed by 9½ A.M.: by 11 A.M. I had finished my observations for horizontal intensity. The small collimator magnet being suspended, the large collimator magnet was placed at four different distances east and west, and the deflecting collimator magnet was then vibrated and 300 oscillations taken.

At 11 A.M. I observed altitudes and azimuths of the sun with the altitude and azimuth instrument for equal altitudes. At noon I took circummeridional altitudes of the sun, three with the limb direct, and three with the limb reversed, for latitude; at 1 P.M. I again took altitudes and azimuths.

By equal altitudes from the mean of the times, I was enabled to check the results given by the sextant for time; and by the azimuths corresponding to the equal altitudes, I checked the observations for the true meridian taken at 9 A.M. for the declination. By this means I was always certain of the results by using different modes of verification.

If I stopped another day at the stations I repeated the observations; if I was going to move off, I packed up the instruments and struck the tents, which generally took me the afternoon and the greater part of the evening, for I had no one to assist me.

At sea, whenever an opportunity offered, I took meteorological observations, viz. the standard thermometer, the dry and wet bulb,

and the temperature of the air and sea at 3 A.M. and P.M., and at 9 A.M. and P.M.; sights for longitude at 9 A.M. and 3 P.M., and latitude at noon, besides the dip three times, and sometimes five times a day; every absolute determination was made by me.

Thus on shore as well as at sea, observations were commenced at 3 A.M., and never terminated till 9 P.M.: I had for my assistant an Indo-Briton.

I will not trouble the Council of the Royal Society with stories of the difficulties I met with; suffice it to say, that a stranger amongst the Dutch, the necessity of conciliating the natives in seeing me employed in a manner so strange to them, travelling in the monsoons and in all weathers, sometimes for hours wet in the saddle, living in huts for weeks, the only shelter being cocoanut leaves, and at sea in a leaky old schooner that was perpetually in danger of foundering, with a captain who was scarcely ever sober,—it is not surprising that once or twice I fell sick. I am now but slowly recovering from Java and Car Nicobar fever caught in the execution of my duty. I take the liberty of adding for the information of the Council of the Royal Society, that I never took a single observation unless I was by myself and my attention undisturbed. If strangers were importunate, I waited until they left me. If the weather was against me, I took no observations until it settled. I made it a rule never to be in a hurry, and always to finish one set of observations before I commenced another, and to be as comfortable as circumstances would admit. I am certain that an observation is the more valuable in proportion to the mind being not only at ease, but able to fix itself with undivided attention on the observations. I never, for instance, would think of taking an observation whilst bored by an intruder, or a high wind, or a heavy shower of rain falling. I preferred under such circumstances invariably to sacrifice the observation rather than to record it.

I have the honour of sending to the Council as a specimen of the way in which the work was carried on, some of the absolute determinations made at the Cocos or Keeling Islands; they will be able to see that often after the labours of the day had commenced at 3 A.M., they were not terminated at 9 P.M.; and in order to observe moon-culminating stars, I had sometimes to remain up the greater part of the night, for I had no one on whom I could place any dependence to awake me at the proper time.

Paper A contains the way and order in which the instruments of the observatory were registered.

Paper B. The dip taken at the Cocos, with an example.

Paper C. The horizontal intensity, with an example.

Paper D. The declination, with an example.

Paper E. The latitude, with an example.

Paper F. The longitude from moon-culminating stars.

I have the honour to be, Sir,

Your most obedient Servant,

Madras,
August 6, 1849.

C. M. ELLIOT,
Captain Madras Engineers.

4. "On the gaseous transformation of Water, by means of a pile in two separate compartments having no other electric communication between them besides conducting wires of copper, and giving, in the one oxygen alone, and hydrogen alone in the other." By M. Daniel Paret. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

After premising that, at the present time, it is the generally received opinion that water is a compound of oxygen and hydrogen, the author states that he now brings forward an experiment which proves, not that water is a compound, but really a simple element, the generator of oxygen and hydrogen, since, without being decomposed, a volume of water being given, it may be entirely transformed at will, either into oxygen or into hydrogen. Thus, he considers, it is no longer a decomposition of pre-existing elements which is effected, but really a gaseous transformation into two "sub-elements" which are formed at the expense of the water, by the transposition of its combined or coercitive electricity which places itself in excess in the water which becomes oxygen, at the expense of another volume which becomes hydrogen. He considers that this will, no doubt, appear very extraordinary, but that nevertheless it is now "un fait accompli et acquis à la science."

After describing the experiments which he considers support his doctrine, the author concludes by observing that these experiments prove,—1st, that contrary to the indefensible theory, a compound electric fluid which is decomposed and recomposed, there is a true transfer of fluid in the current, which besides would be sufficiently evident by its motive power. 2nd. That the electric fluid is really the coercitive agent of cohesion. 3rd. That water is not a compound, is not an oxide, but truly a first element, the generator of oxygen and of hydrogen. 4th. In fine, it reveals a power unknown until now, and that very likely many other bodies are in the same case as water.

In a second communication the author states that he had occasionally met with some anomalous results on repeating his experiments, and then points out how these are to be obviated.

January 31, 1850.

LIEUT.-COLONEL REID, R.E., Vice-President, in the Chair.

The following papers were read:—

1. "An account of a remarkable Aurora Borealis seen at Montreal on the 13th of August 1849." By Mr. Thomas McGinn. Communicated by Thomas Bell, Esq., Sec. R.S., &c.

The author having witnessed a singular aurora on the 13th of August, in this communication gives a description of the phænomenon. He states that, on the evening in question, the whole northern hemisphere was screened by thick dark clouds, which, though very small, were closely packed together. Shortly after sunset (7^h 34^m) it became quite dark, and at 8 o'clock the existence of the

meteor was indicated by a mellow luminous tinge which appeared through the openings of the clouds in the north.

About half-past eight a similar luminous glow was observed through the clouds which were fast disappearing in a heavy dew. This light appeared like a belt of 2° broad, extending across the sky from a point almost due east directly to the west, and reaching within 5° or 6° of the horizon. As the clouds disappeared, which they did very rapidly, the true character of the aurora became more perfectly developed. In the north the usual dark arch from which the columns of light ordinarily appear to issue, was for the greater part of the time wanting; and the luminous columns seemed to rise from the earth, extending upwards occasionally to the pole star, beyond which no trace of them was visible. A brown vapoury cloud, the only one now visible, extended along the horizon from N.N.E. to a few points south of east, and maintained apparently a motionless position, the lower part appearing to rest upon the earth, and the upper edges, which seemed uniform, rose about 6° above the horizon. Immediately in the east, and apparently issuing from this cloud, rose the belt or zone of light already noticed, forming a magnificent arch. The light emitted from this zone was of a milky whiteness, and the matter of it seemed to be much more compact than any portion of an aurora ever seen by the author; but immediately in the zenith, where it intersected the Milky Way, it appeared to be far less compact. At this point, where alone motion was observable, a constant current was seen, presenting the appearance of light fleecy clouds driven by a strong wind, and following each other in such close succession as to appear in contact. This stream of the aurora was maintained undiminished for more than an hour, during which time the eastern part of the zone did not appear to lose either in volume or brilliancy, nor did the western seem to gain in either of these respects. After an hour, the dark cloud seemed to diminish slowly, and with it the zone began to lose its brilliancy. In about another hour this cloud and also the zone, which throughout had maintained apparent contact with it, vanished. The conclusion, that the dark cloud served the purpose of a conductor and fed the zone drawing off the matter of the aurora from the north, seemed to the author inevitable. The cloud did not appear to him to be more than forty or fifty miles distant. In conclusion he remarks that none of the prismatic tints were observable on this occasion.

2. "On the Development of the Retina and Optic Nerve, and of the Membranous Labyrinth and Auditory Nerve." By Henry Gray, Esq., M.R.C.S. Communicated by William Bowman, Esq., F.R.S., &c.

The author has divided the observations contained in this paper into two parts:—the first of which treats of "The Development of the Retina and Optic Nerve; the second, of the Development of the Membranous Labyrinth and Auditory Nerve."

In the observations on the development of the retina, which have

been made on the embryo of the chick, the author demonstrates its mode of evolution, and also the mode of development of the various layers of which this membrane is formed. They commence at the early period of the thirty-third hour of incubation, at which time the cephalic extremity of the embryo presents a slight protrusion of its walls, which by the thirty-sixth hour is very considerably increased, having become more elongated and protruded outwards, presenting a somewhat dilated end, and being somewhat constricted at its connection with the anterior cerebral cell from which it arises. This is the first indication of the development of this membrane.

At the forty-sixth hour this protrusion (which the author calls the optic vesicle) was still more distinct, and the cavity in the cerebral cell, from the wall of which it arises, was well seen, and it was observed to communicate with the cavity of the optic vesicle which was also hollow. This description of the mode of development of the retina the author considers as confirmatory of the observations made by Baër, but not in accordance with that given by Wagner or Huschke.

The author then proceeds to detail very minutely the consecutive stages of the development of the retina and parts in immediate connection with it, until the seventh day, when he states that on making a section of the eye, it was separated from the other tunics as a perfectly distinct layer. The optic nerves were also now completely formed, being united to form the chiasma, and passed inwards in the direction of the under surface of the corpora quadrigemina.

The author in the next place proceeds to consider the development of the various layers of the retinal membrane, a point which appears not to have been previously noticed by any physiologist. This membrane on the eighth day of incubation can be seen, by the naked eye, distinct from the other tunics. Its choroidal surface is composed of a mass of globular nuclei about the size of the red corpuscles of the blood, which form at this period about one-half the entire thickness of the retina, the deeper surface consisting of some fine granular matter and a mass of pale and delicate nucleated cells similar to those found surrounding the fibrous lamina in the normal structure of the membrane.

The "Membrana Jacobi" is first observed on the thirteenth day as a fine pale granular stratum which covers in the globular nuclei already described. In this, at about the fifteenth day, some brilliant yellowish granules are imbedded; they vary in size from the 5000th to the 8000th of an inch, and around them a delicate cell-wall is traced; they soon acquire an oval shape; then become more elongated; and about the eighteenth day the almost perfect rods are formed. They are now disposed in an imbricated manner, and their nuclei, which are of a bright yellow colour, are placed generally at the apex, but sometimes in the middle of the rods. On the twenty-first day this membrane is similar to what is seen in the full-grown bird.

The first trace of the "fibrous lamina" is seen between the fourteenth and fifteenth days, as a fine pale granular lamina marked by

numerous faint longitudinal striæ. On the eighteenth day this membrane when separated from the other layers is seen composed of numerous fibrillated meshes, in which are deposited the nucleated vesicles which are formed as early as the eighth day. From these observations it is seen that the retina is formed as a protrusion from the most anterior cerebral cell, being hollow and communicating with its cavity; that it subsequently assumes a pyriform shape, presenting a dilated end, the future retina, and a tubular portion, the optic nerve. As the tubular portion becomes solidified so as to form the optic nerve, then no communication can be traced between the optic vesicle and the cavity from which it is an offset. By degrees the spherical end of the protrusion is absorbed, and the retina, being now fully formed, becomes attached to the margin of the lens. The optic nerve is then traced to be connected not only with the anterior cerebral cell, but, uniting with its fellow at the under surface of the optic lobes, is seen partly to terminate in those bodies. The deductions from these observations may be thus briefly stated:—

1st. They confirm the observations on the structure of the retina made by Bowman, who has shown that the essential part of this membrane is analogous to the cineritious matter of the brain, and is composed like it of a fibrous mesh intermingled with vesicles of grey matter, being, in fact, a portion of the cerebrum pushed outwards and connected with the brain by its appropriate commissure, the optic nerve. The mode of development of this membrane would show this to be the correct view of the structure of this essential part of the retinal expansion, and at the same time disprove the statements of Henle, who believed it to be more analogous to epithelium.

2nd. The origin of the optic vesicle from the anterior cerebral cell, would show the incorrectness of the opinion of those anatomists who have stated that none of the fibres of the optic nerve could be traced to the optic thalami. The thalami being developed from the same centre from whence these vesicles arise, would render it exceedingly probable that the optic nerves had some connection with those bodies.

The second part of the paper describes the development of the membranous labyrinth and auditory nerve.

The essential part of the auditory apparatus, viz. the membranous labyrinth, consists, like the retina, of a membranous lamina formed of the terminal axes cylinders of the nerve tubules in intimate connection with a layer of closely-set nucleated cells; like it also, it may be regarded as a portion of the brain protruded outwards, and connected with an appropriate apparatus which receives and transmits its peculiar impressions; its mode of development also shows a striking analogy between it and the retinal expansion.

At the fiftieth hour of incubation, there is seen on either side of the medulla oblongata, (which is not closed in above and presents an open shallow cavity,) the first rudiment of the auditory sac, in the form of a small circular-shaped protruded vesicle, communicating with the ventricular cavity from the lateral wall of which it

is an offset. The vesicle was hollow, clear and pellucid, and of a flattened circular form. At the fifty-sixth hour it had increased in size and presented a pear-shaped figure; so that now the narrow contracted tubular portion appeared the first stage in the development of the auditory nerve; the dilated portion, the auditory sac or rudimentary vestibule; and the cavity still existing in its interior and communicating with the ventricular cavity from which it arises, by means of the tubular prolongation, the auditory nerve. The aperture of communication soon becomes smaller and more contracted, and this increases as the separation between the auditory vesicle and its parent-cell takes place. At the sixty-fifth hour, besides a great increase in the size of the ear-bulb, the auditory nerve has become more distinctly formed, and is quite solidified, so that no communication can now be traced between the ventricular cavity and the vestibular sac. It is in this stage of the development of the auditory apparatus that a great similarity is to be observed between it and the normal condition of the same part in some of the lower animals. There are, in fact, now formed the two elementary portions of the auditory apparatus, the auditory nerve and the simple vestibular sac. Such is the simple condition of the organ in the Crustacea and Cephalopod Mollusks. At the seventy-second hour, the vestibular sac has lost its oval form and presents a contraction around its entire circumference. This is the first indication of the separation of the vestibule from the membranous semicircular canals which are ultimately formed from the terminal portion of the vesicle.

The minute examination of the development of these structures, of which a consecutive detail is given, leads the author to remark on the almost precise similarity in structure of the membranous labyrinth to the retina in its various stages of development, for it consists like it of a delicate fibrous mesh in the areolæ of which is deposited granular matter and numerous nucleated cells, its outer surface being composed of globular-shaped nuclei arranged similar to those covering the outer surface of the retina at an early period of its development.

From this description a marked similarity may be observed between the origin of this membrane and that of the retina. In both cases they arise as a protruded portion of the cerebral mass, being hollow and communicating with the cavity of the parent-cell. In process of time, a gradual separation takes place between them and the parts from whence they arise. They then assume a pyriform shape, but still communicate with the cerebral cavity. As, however, the nerves become solidified and the separation between them is more fully effected, then no communication can be traced between the two cavities.

3. "Tide Researches. Fourteenth Series. On the Results of continued Tide Observations at several places on the British Coasts." By the Rev. W. Whewell, D.D., F.R.S. &c.

Tide observations made at several different parts of the British

and neighbouring shores, and in some instances continued for a considerable period, having been discussed by Mr. D. Ross of the Hydrographer's Office, with great labour and perseverance, a brief statement of the results which his labours afford is here presented by Dr. Whewell.

The discussions here referred to relate to the height of high water, and the variations which this height undergoes in proceeding from springs to neaps, and from neaps to springs. It is found, by examining the observations at 120 places, and throwing the heights into curves, that the curve is very nearly of the same form at all these places. Hence the semi-mensual series of heights at any place affords a rule for the series of heights at all other places where the difference of spring height and neap height is the same. For instance, Portsmouth, where the difference of spring height and neap height is 2 ft. 8 in., is a rule for Cork, Waterford, Inverness, Bantry, Arcachan on the French coast, and other places: and the tables of the heights of high water at one of these places suffices for all the others, a constant being of course added or subtracted according to the position of the zero-point from which the heights at each place are measured.

The series of heights of high water for a semi-lunation also agrees very exactly, as to the form of the curve, with the equilibrium theory. A very simple construction is given for the determination of this curve. The properties deduced according to theory from this construction are, however, in actual cases, modified in a manner which is then described.

1. The tides in these discussions are not referred to the transit of the moon immediately preceding, but to some earlier transit, namely, the second, third, fourth or fifth preceding transit, it being found that in this way the accordance with the theory becomes more exact.

2. According to this construction, the difference of springs and neaps would be to the height of neaps above low water springs as 10 to 24, a constant ratio for all places; but in fact this ratio is different at different places: and the observations under consideration show that the ratio is smaller where the tide is smaller.

In consequence of the law of the high water, given alike by the theory and by the observations, the spring high waters are above the mean high water for a longer period than the neaps are below it.

February 7, 1850.

Sir BENJAMIN C. BRODIE, Bart., Vice-President, in the Chair.

The following papers were read:—

1. "On the development and homologies of the Molar Teeth of the Wart-Hogs (Phacochærus), with illustrations of a System of

Notation for the Teeth in the Class Mammalia." By Richard Owen, Esq., F.R.S. &c.

The author commences by a brief statement of the facts and conclusions recorded in a paper by Sir Ev. Home on the dentition of the *Sus Æthiopicus*, in the Philosophical Transactions for 1799, p. 256; and gives the results of an examination of the original specimens described and figured by Home, and of other specimens showing earlier stages of dentition, which lead to the following conclusions as to the number, kinds, and mode of succession of the teeth in the genus *Phacochærus*. The tooth answering to the first milk-molar and first premolar in the upper jaw, and those answering to the first and second milk-molars and corresponding premolars in the lower jaw of the common Hog are not developed. Eight successive phases of development of the grinding teeth of the African Wart-hogs are described and expressed by the following notation:—

Phase.	No. of grinding teeth.		Kinds of teeth.
I.	$\frac{5-5}{4-4}$	viz.	$\begin{cases} d\,2, d\,3, d\,4, m\,1, m\,2. \\ d\,3, d\,4, m\,1, m\,2. \end{cases}$
II.	$\frac{6-6}{5-5}$	viz.	$\begin{cases} p\,2, p\,3, p\,4, m\,1, m\,2, m\,3. \\ p\,3, p\,4, m\,1, m\,2, m\,3. \end{cases}$
III.	$\frac{5-5}{4-4}$	viz.	$\begin{cases} p\,3, p\,4, m\,1, m\,2, m\,3. \\ p\,4, m\,1, m\,2, m\,3. \end{cases}$
IV.	$\frac{4-4}{4-4}$	viz.	$p\,4, m\,1, m\,2, m\,3.$
V.	$\frac{4-4}{3-3}$	viz.	$\begin{cases} p\,3, p\,4, m\,2, m\,3. \\ p\,4, m\,2, m\,3. \end{cases}$
VI.	$\frac{3-3}{3-3}$	viz.	$p\,4, m\,2, m\,3.$
VII.	$\frac{2-2}{2-2}$	viz.	$p\,4, m\,3.$
VIII.	$\frac{1-1}{1-1}$	viz.	$m\,3.$

These observations prove that, contrary to the opinion of Home and Cuvier, the Wart-hogs have deciduous teeth, succeeded vertically by premolar teeth; in the *Phacochærus Æliani*, at least, three deciduous teeth are, in some individuals, succeeded by as many premolar teeth; and, as a general rule, two deciduous teeth are displaced vertically by two premolars. The first true molar is remarkable for its unusually early development, which is followed by an unusually early abrasion and expulsion, when its place is obliterated by the second true molar being pushed forwards into contact with the last premolar. This tooth is as remarkable for its longevity, and remains after the wearing away and shedding of the second true molar, when the last true molar advances into contact

with the last premolar, and the place of both the previously intervening true molars is obliterated. This unusual order of shedding of the molar teeth has given rise to the idea of the last large and complex true molar of the *Phacochœrus* being the homologue of both the last and penultimate grinders of the common Hog, which the author's observations refute; and he, also, is able to point out, by re-examination of the original specimen figured by Home in the Phil. Trans., the source of the erroneous idea that the common Hog had an additional true molar behind the large one symbolised by *m* 3, in the author's system of dental notation.

The nature and signification of the symbols proposed are explained and illustrated by a series of drawings. One of the fruits of the determination of the homology of a part is the power of giving it a name, and signifying it by a symbol applicable co-extensively with such homology. The limits are shown within which the homologies of individual teeth can be determined: they present the requisite constancy of character in a large proportion of the class Mammalia. Certain members of this class, *e.g.* the order *Bruta* and the *Cetacea vera*, have teeth too numerous and alike in form and mode of development to admit of being determined individually from species to species. Such mammalia have but one set of teeth, and the author proposes to call them 'Monophyodonts.' On the other hand, the orders *Marsupialia*, *Insectivora*, *Rodentia*, *Ruminantia*, *Pachydermata*, *Carnivora*, *Cheiroptera*, *Quadrumana* and *Bimana* have two sets of teeth, and might be called collectively, 'Diphyodonts.' Of the permanent teeth of this division of mammalia, some succeed the deciduous teeth vertically, others come into place behind one another in horizontal succession. The 'incisors' are determined by a character of relative position to the jaws and to each other: so likewise the 'canines.' The remaining teeth are divided into those which are developed in vertical relation to the deciduous molars, and push them out, and those that have not such relation, but follow each other horizontally: the term 'molar' is restricted by the author to these latter teeth, and that of 'premolar' to the former ones, which are always anterior to the molars. There is a remarkable degree of constancy in the number of these different kinds of teeth; in the placental Diphyodonts, *e.g.* the 'incisors' never exceed $\frac{3-3}{3-3}$, *i.e.* 3 on each side of both jaws, the 'canines' $\frac{1-1}{1-1}$, the premolars $\frac{4-4}{4-4}$, the molars $\frac{3-3}{3-3}$, $=44$; and this the author regards as the typical formula of dentition in the great proportion of the mammalian class above defined. It was rarely departed from by the primæval species that have become extinct, and is modified chiefly by defect or loss of certain teeth in the existing species. When the grinders are below the typical number, the missing molars are taken from the back part of their series, and the premolars from the fore part of theirs: the most constant teeth being the fourth premolar and first true molar; these are always determinable, whatever be their form, by the relation to them of the

last tooth of the deciduous series. Thus determined, the homologies of the other grinders are ascertained by counting the molars from the first backwards, 1, 2, 3; and the premolars from the last forwards, 4, 3, 2, 1. The symbols are made by adding the initial *m* to the numbers of the molar teeth, and the initial *p* to those of the premolar teeth. The author concludes by pointing out the advantages of this system of anatomical notation.

2. "Description of the Hydrostatic Log." By the Rev. E. L. Berthon, M.A. Communicated by Sir Francis Beaufort, F.R.S. &c., on the part of the Lords Commissioners of the Admiralty.

The object of this invention is to obtain a register of the speed of ships, by a column of mercury, in such a manner that the height of the column shall depend upon the velocity alone, and not be affected by any disturbing causes, such as alteration of draught of water, pitching and rolling, &c.

The principle embraces that of Pitôt's tube, inasmuch as the force of the resistance due to the velocity is communicated through a small pipe projecting into the water below the bottom of the ship: this force, acting upwards, compresses a portion of enclosed air in a small cylinder, which air communicating by means of a little pipe with the bulb of a glass tube—bent like a common barometer—raises the mercury in the tube, by depressing it in the bulb.

But as any single column of water and air thus acting upon the surface of the mercury in the *bulb alone* must depend not only upon the resistance due to the velocity, but also upon the *distance of the cylinder from the water-line,* which distance or height varies with every sea, and alters more permanently as the draught of water changes, a compensation was necessary; and the inventor has found one, which he considers perfect for all these variations, by applying *a second column of water and air* to press upon the *other surface of the mercury,* viz. that in the glass tube. This second column is precisely like the first as regards the pipe and cylinder, and communicates with the sea by an aperture or apertures, presented in such a direction that velocity does *not produce any increase of pressure.* Thus the mercury in the indicator is placed *between* TWO *columns* of water and air, which are *always equal to each other* in length, and the mercury rises according to the *difference* between the pressures upon its two surfaces, the result of resistance or velocity alone.

The air-pipes may be conducted in any direction, and the indicator, which swings upon gimbals, may be placed in any part of the ship. The two water-pipes are conducted into one tube in the bottom of the ship, divided into two separate chambers for the different forces.

In addition to the speed, the true course or leeway of the vessel is indicated upon a horizontal segment divided into degrees, over which a needle is moved by a rod connected with the above-mentioned double tube; and the whole is kept continually in the true direction of the ship's motion by a float or vane attached to the lower end of the tube in the water.

February 14, 1850.

Sir RODERICK I. MURCHISON, Vice-President, in the Chair.

The following papers were read:—

1. "Supplementary Observations on the Structure of the Belemnite and Belemnoteuthis." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society, &c.

In this communication the author describes his recent investigations on the structure of the two genera of fossil Cephalopoda, whose remains occur so abundantly in the Oxford clay of Wiltshire, namely, the Belemnite and Belemnoteuthis, as supplementary to his memoir on the same subject, published in the Phil. Trans. 1848. In that paper evidence was adduced to show the correctness of the opinion of the late Mr. Channing Pierce as to the generic distinction of these two extinct forms of Cephalopoda.

As however several eminent naturalists had expressed doubts as to some of the opinions advanced by the author in his former memoir, figures and descriptions are given in the present notice, of beautiful and instructive specimens lately discovered in Wiltshire, and which he conceives establish his previous conclusions. Dr. Mantell then states as the result of his examination of several hundred examples, that our knowledge of the organization of the animal of the Belemnite is at present limited to the following parts, viz.—

1. An external *Capsule* or *periostracum* which invested the osselet or sepiostaire, and extending upwards, constituted the external sheath of the receptacle.

2. The *Osselet*, characterized by its fibrous radiated structure, terminating distally in a solid rostrum or guard, having an alveolus, or conical hollow, to receive the apical portion of the chambered phragmocone; and expanding proximally into a thin cup, which became confluent with the capsule, and formed the receptacle for the viscera.

3. The *Phragmocone*, or chambered, siphunculated, internal shell; the apex of which occupied the alveolus of the guard, and the upper part constituted a capacious chamber, from the basilar margin of which proceeded two long, flat, testaceous processes.

These structures comprise all that are at present known of the animal to which the fossil commonly called "*The Belemnite*," belonged.

Of the *Belemnoteuthis*, the fossil cephalopod which Prof. Owen regards as identical with the Belemnite, many examples of the body with eight uncinated arms, and a pair of long tentacula, having an ink-bag and pallial fins, have been discovered. The osselet of this animal, like that of the Belemnite, has a fibro-radiated structure, investing a conical chambered shell; but this organ, for reasons fully detailed in the memoir, the author considers could never have been contained within the alveolus of a Belemnite; the soft parts of the animal of the Belemnite are therefore wholly unknown.

Many beautiful specimens of Belemnites and Belemnoteuthis were

exhibited by Dr. Mantell to the Society, in proof of the statements contained in the memoir.

2. "On the PELOROSAURUS; an undescribed gigantic terrestrial reptile, whose remains are associated with those of the Iguanodon and other Saurians, in the Strata of Tilgate Forest." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., Vice-President of the Geological Society, &c.

The author had for a long while entertained the idea, that among the remains of colossal reptiles obtained from the Wealden strata, there were indications of several genera of terrestrial saurians, besides those established by himself and other geologists. The recent discovery of an enormous arm-bone, or humerus, of an undescribed reptile of the crocodilian type, in a quarry of Tilgate Forest in Sussex, where Dr. Mantell had many years since collected numerous teeth and bones of the Iguanodon, Hylæosaurus, &c., and some remarkable vertebræ not referable to known genera, induced him to embody in the present communication the facts which his late researches have brought to light.

The humerus above-mentioned was found imbedded in sandstone, by Mr. Peter Fuller of Lewes, at about 20 feet below the surface; it presents the usual mineralized condition of the fossil bones from the arenaceous strata of the Wealden. It is four and a half feet in length, and the circumference of its distal extremity is 32 inches! It has a medullary cavity 3 inches in diameter, which at once separates it from the Cetiosaurus and other supposed marine saurians, while its form and proportions distinguish it from the humerus of the Iguanodon, Hylæosaurus, and Megalosaurus. It approaches most nearly to the Crocodilians, but possesses characters distinct from any known fossil genus. Its size is stupendous, far surpassing that of the corresponding bone even of the gigantic Iguanodon; and the name of *Pelorosaurus* (from πέλωρ *pelor*, monster) is therefore proposed for the genus, with the specific term *Conybeari*, in honour of the palæontological labours of the Dean of Llandaff.

No bones have been found in such contiguity with this humerus, as to render it certain that they belonged to the same gigantic reptile; but several very large caudal vertebræ of peculiar characters, collected from the same quarry, are probably referable to the Pelorosaurus; these, together with some distal caudals which belong to the same type, are figured and described by the author.

Certain femora and other bones from the oolite of Oxfordshire, in the collection of the Dean of Westminster, at Oxford, are mentioned as possessing characters more allied to those of the Pelorosaurus, or to some unknown terrestrial saurian, than to the Cetiosaurus, with which they have been confounded.

As to the magnitude of the animal to which the humerus belonged, Dr. Mantell, while disclaiming the idea of arriving at any certain conclusions from a single bone, states that in a Gavial 18 feet long, the humerus is 1 foot in length; *i.e.* one-eighteenth part of the length of the animal, from the end of the muzzle to the tip of

the tail. According to these admeasurements the Pelorosaurus would be 81 feet long, and its body 20 feet in circumference. But if we assume the length and number of the vertebræ as the scale, we should have a reptile of relatively abbreviated proportions; even in this case, however, the original creature would far surpass in magnitude the most colossal of reptilian forms.

In conclusion, Dr. Mantell comments on the probable physical conditions of the countries inhabited by the terrestrial reptiles of the secondary ages of geology. These highly-organized colossal land saurians appear to have occupied the same position in those ancient faunas as the large mammalia in those of modern times. The trees and plants whose remains are associated with the fossil bones, manifest, by their close affinity to living species, that the islands or continents on which they grew possessed as pure an atmosphere, as high a temperature, and as unclouded skies as those of our tropical climes. There are therefore no legitimate grounds for the hypothesis in which some physiologists have indulged, that during the "*Age of Reptiles*" the earth was in the state of a half-finished planet, and its atmosphere too heavy, from an excess of carbon, for the respiration of warm-blooded animals. Such an opinion can only have originated from a partial view of all the phenomena which these problems embrace, for there is as great a discrepancy between the existing faunas of different regions, as in the extinct groups of animals and plants which geological researches have revealed.

The memoir was illustrated by numerous drawings, and the gigantic humerus of the Pelorosaurus and other bones were placed before the Society.

February 21, 1850.

GEORGE RENNIE, Esq., Treasurer, in the Chair.

Robert Alfred Cloyne Austen, Esq. was admitted into the Society.

The following papers were read:—

1. "On the Extension of the Principle of Fermat's Theorem of the Polygonal Numbers to the higher orders of series whose ultimate differences are constant. With a new Theorem proposed, applicable to all the Orders." By Sir Frederick Pollock, Lord Chief Baron, F.R.S.

The object of this paper professes to be to ascertain whether the principle of Fermat's theorem of the polygonal numbers may not be extended to all orders of series whose ultimate differences are constant. The polygonal numbers are all of the *quadratic* form, and they have (according to Fermat's theorem) this property, that every number is the sum of not exceeding, 3 terms of the triangular numbers, 4 of the square numbers, 5 of the pentagonal numbers, &c.

It is stated in this paper that the series of the odd squares 1, 9, 25, 49, &c. has a similar property, and that every number is the sum of

not exceeding 10 odd squares. It is also stated, that a series consisting of the 1st and every succeeding 3rd term of the triangular series, viz. 1, 10, 28, 35, &c., has a similar property; and that every number is the sum of not exceeding 11 terms of this last series, and that this may be easily proved [it was proved in a former paper by the same author]. The term "Notation-limit" is applied to the number which denotes the largest number of terms of a series necessary to express any number; and the writer states that 5, 7, 9, 13, 21 are respectively the notation-limits of the tetrahedral numbers, the octohedral, the cubical, the eicosahedral and the dodecahedral numbers; that 19 is the notation-limit of the series of the 4th powers; that 11 is the notation-limit of the series of the triangular numbers squared, viz. 1, 9, 36, 100, &c., and 31 the notation-limit of the series 1, 28, 153, &c. (the sum of the odd cubes), whose general expression is $2n^4-n^2$.

The paper next contains an extension of the theorem $8n+3=3$ odd squares, which was proved by Legendre in his *Théorie des Nombres*; every odd square equals 8 times a triangular No.+1; the theorem therefore is—8 times any term in the figurate series (1, 2, 3, 4, &c. ..) +3=3 terms of a series consisting of the next series, viz. (1, 3, 6, 10 . . &c.), multiplied by 8 with 1 prefixed, and also added to each term. But it is stated that this theorem may be much extended; for this is not only true of any two consecutive series, but generally if F_x represent any figurate number of the x^{th} order, and F_y any figurate number of the y^{th} order, whether y be greater or less than x,

$$8F_x+3=3, \text{ or } (3+8), \text{ or } (3+2\,.\,8), \text{ or} \ldots (3+n8), \&c.,$$

terms of a series whose general expression is $8F_y+1$; and still further (provided p be greater than 2)—

$$pF_x+3=3, \text{ or } (3+p), \text{ or } (3+2p), \text{ or } (3+np),$$

terms of a series whose general expression is pF_y+1, and *vice versâ*.

The author concludes from these considerations, that probably there are many theorems which are common to all the orders. The following theorem is then proposed as having that character.

If the terms of a series be

$$1, \text{ or } (1+n)^0, (1+n)', (1+n)^2 \ldots \&c.\ (1+n)^p,$$
$$\text{the 1st } (p+1) \text{ terms of } (1+n)^{p+1}$$
$$\text{the 1st } (p+1) \text{ terms of } (1+n)^{p+2}$$
$$\text{the 1st } (p+1) \text{ terms of } (1+n)^{p+3}$$
$$+ \quad \&c. \quad \&c.$$

(if p and n be both not less than 1), any number will be the sum of not exceeding $(pn+1)$ terms of the series; in other words, $pn+1$ is the notation-limit of this series.

It is manifest that this series is of such a form, that by varying n and p, it is capable of expressing every possible arithmetical series, also every possible geometrical series (each having 1 for the first term); it will also express all the intermediate series of the successive orders (to an indefinite extent), which exist between and con-

nect together by a regular gradation (as is well known) any such arithmetical series with a geometric series, whose common ratio is the 2nd term of both series. The theorem may be stated without the series thus:—

If any geometric series (having 1 for its first term) and $(1+n)$ for its common ratio, be stayed at the $(p+1)$th term and discontinued as a *geometric* series, but be continued from that term as an *arithmetic* series of the *pth* order, by forming it with the *pth* difference as the constant difference, and the other differences (which will be x, x^2, x^3, &c. . . . , x^p). The resulting series will be the series stated in the theorem above, and any number may be formed by not exceeding $(pn+1)$ terms, that is $(pn+1)$ will be the notation-limit of the series; if p becomes indefinitely great, the limit of the series is a geometrical series, and it would become capable of expressing any number according to a system of notation whose base or local value would be $(1+n)$.

The proof of the theorem seems to depend upon this, that the notation-limit assigned by the theorem is actually the notation-limit of all the geometric terms and one more, at least, while the geometric terms alone fix the law of the series and ascertain its *elements* (that is, the first term and the successive differences); and as the combinations necessary to enable the series to fulfill its law, and carry on the notation that belongs to it, are regulated by the series next below it, viz. by the first rank of differences, while the supply of new combinations (as the series advances and the number of terms that may be used increases) is indicated by even a higher series than itself, the new combinations are always greater, and at length indefinitely greater, than the number required. If therefore within the range of those terms that ascertain and fix the law of the series the law of its notation-limit can be obeyed, it must always (*à fortiori*) be obeyed as the series proceeds to a greater number of terms and to a variety of combinations increasing in a higher ratio; and the series will furnish the numbers requisite to carry on the notation by the new and more numerous combinations which must of necessity be of the same numerical kind with those which have preceded them. It is shown at length, that the new combinations, as the series advances, do actually increase in an increasing proportion compared with the numbers required.

2. "Experiments on the section of the Glossopharyngeal and Hypoglossal Nerves of the Frog, and Observations of the alterations produced thereby in the Structure of their primitive fibres." By Augustus Waller, M.D. Communicated by Professor Owen, F.R.S. &c.

After describing the natural structure of the tubular fibres of the nerves, the author states the results which he observed to follow the section of the nerves of the frog's tongue. To this organ two principal pairs of nerves are distributed; one of these, issuing from the cranium along with the pneumogastric and distributed to the fungiform papillæ, is regarded as the glossopharyngeal; the other,

arising from the anterior part of the spinal cord, and passing through the first intervertebral foramen, the author (following Burdach) names the hypoglossal. Section of the glossopharyngeal nerves does not cause any perceptible loss of motion or of common sensation, and this fact, together with its distribution to the fungiform papillæ, leads the author to consider this nerve as the nerve of tasting. On the other hand, when the hypoglossal nerves are divided, the tongue is no longer sensible to mechanical irritation, and its motions are entirely abolished. Simultaneous division of the right and left glossopharyngeal nerves is followed by the death of the animal in a few days, and the same effect ensues after division of both hypoglossals. This result, which takes place more speedily in summer than in winter, the author is disposed to ascribe to a disturbance of the mechanical process of respiration, in which, as is well known, the muscles of the frog's mouth and tongue take a large share.

To ascertain the changes which take place in the nerve-fibres after division of the trunks to which they belong, the operation was confined to the nerve of one side only, and the fibres of the uninjured nerve of the other side served for comparison. These changes ensue more speedily, and go on more rapidly in summer than in winter, commencing usually in about five days. The pulp contained in the tubular nerve-fibres, originally transparent, becomes turbid, as if it underwent a sort of coagulation, and is soon converted into very fine granules, partly aggregated into small clumps, and partly scattered within the tubular membrane. These granules are at first abundant, and render the nerve-fibre remarkably opaque; but in process of time they diminish in number, and, together with the inclosing membrane, at length disappear, so that at last the finest ramifications of the nerves which go to the papillæ, or those going to the muscular fibres of the tongue (according to the nerve operated on), are altogether lost to view, in consequence of the destruction and evanescence of their elementary fibres. The disorganization begins at the extremities of the fibres, and gradually extends upwards in the branches and trunk of the nerve. The other tissues of the tongue remain unaltered. When the cut ends of the nerve are allowed to reunite, the process of disorganization is arrested, and the nervous fibres are restored to their natural condition. The author ascribes the disorganization and final absorption of the nerve-fibres to an arrestment of their nutrition caused by interruption of the nervous current, and considers his experiments as affording most unequivocal evidence of the dependence of the nutrition on the nervous power.

February 28, 1850.

PETER MARK ROGET, M.D., Vice-President, in the Chair.

The following papers were read:—

1. "Sequel to a Paper on the reduction of the Thermometrical

Observations made at the Apartments of the Royal Society, with an Appendix." By James Glaisher, Esq., F.R.S.

The principal object of this paper is the connexion of the results deduced in a former paper from the observations at the Royal Society's Apartments, with the observations at the Royal Observatory at Greenwich, in order to determine mean numerical values, and to establish the laws of periodic variation from this long series of observations; the two series of observations are here reduced to one and the same series.

The observations at the Royal Society having been discontinued between the years 1781 and 1786, it was necessary to supply this link in the series, more particularly as these years were distinguished by very severe weather, and their omission would have a sensible effect on the results. The deficient observations have been supplied by a comparison of the observations which were made at Somerset House, with the observations during the corresponding years made by Mr. Barker at Lyndon in Rutlandshire, from 1771 to 1799, corrections being thus obtained for reducing the Lyndon observations to those at Somerset House.

By a comparison of the temperature of the air at Somerset House and at the Royal Observatory for every month during the years 1833 to 1843, corrections necessary to be applied for reducing the mean values at the one place to those at the other, are deduced.

Thus the results of the observations at Somerset House are reduced to those at the Royal Observatory, and a table is given showing the mean temperature at the latter place of each month in every year from 1771 to 1849 inclusive. By taking the means of the several columns in this table, the mean temperature of each month is deduced from all the observations. From these mean monthly temperatures a table is constructed showing the excess of the mean monthly temperature at Greenwich for each year, above the temperature of the month deduced from all the years.

Tables are next given showing the mean temperature in quarterly periods for the year, and for successive groups of years at the Royal Observatory at Greenwich, from the year 1771 to 1849; and the excess of the quarterly and yearly mean temperatures in every year, and for groups of years, above the means from all the years. The author remarks that the numbers in these tables do not at all confirm the idea that a hot summer is either preceded or followed by a cold winter, or *vice versâ*; but on the contrary it would appear that any hot or cold period has been mostly accompanied by weather of the same character, and instances are cited in support of this conclusion.

Tables are also given, based upon the readings of the self-registering thermometers, exhibiting the extreme readings at Somerset House and at the Royal Observatory.

Incidentally the author goes into an inquiry respecting the relative temperature of London and the country in its neighbourhood. From the observations made by Mr. Squire at Epping from 1821 to 1848, and also from those at Lyndon, he concludes that the general fact of a higher winter temperature and lower summer temperature at

the Royal Society's Apartments is satisfactorily proved, and that the same cause has been in operation at both seasons; this cause he considers to be the vicinity of the river Thames to the place of observation. With the view of showing the extent to which this cause operates, a table is given of the mean monthly temperature of the water of the Thames, and a comparison is made between the results of observations made on board the 'Dreadnought' Hospital Ship, at the height of 32 feet above the water, with simultaneous observations at the Royal Observatory. From this comparison it is concluded, that at all seasons of the year the temperature at the 'Dreadnought' is above that at the Observatory during the night hours, and that it is below during the mid-day hours only: at times of extreme temperature the effects of the water upon the temperature of the air is very great.

The paper is accompanied by diagrams exhibiting to the eye, by means of coordinates, the numerical results given in the tables.

2. "On the Communications between the Tympanum and Palate in the Crocodilian Reptiles." By Richard Owen, Esq., F.R.S. &c.

After citing the descriptions by Cuvier, Kaup, Bronn, and De Blainville of the Eustachian tubes and the foramina in the base of the cranium of the recent and extinct Crocodiles, the author gives an account of the nerves, arteries, veins and air-tubes that traverse these different foramina, and thus determines the true position of the carotid foramina and posterior nostrils in the *Teleosauri* and other fossil *Crocodilia*, which had been a matter of controversy amongst the authors cited. In the course of these researches the author discovered a distinct system of Eustachian canals superadded to the ordinary lateral Eustachian tubes, which he describes as follows:—

"From each tympanic cavity two passages are continued downwards, one expands and unites with its fellow from the opposite side to form a median canal which passes from the basisphenoid to the suture between that and the basioccipital, where it terminates in the median canal continued to the orifice described by M. De Blainville as the posterior nostril. The second passage leads from the floor of the tympanic cavity to a short canal which bends towards its fellow, expands into a sinus and divides: one branch descends and terminates in the small lateral foramen at the lower end of the suture between the basioccipital and the basisphenoid: the other branch continues the course inwards and downwards until it meets its fellow at the median line of the basioccipital, and it forms the posterior primary division of the common median canal: this soon joins the anterior division, and the common canal terminates at the median opening below. Membranous tubes are continued from the three osseous ones, and converge to terminate finally in the single Eustachian orifice on the soft palate behind the posterior nostril. The mucous membrane of the palate lines the various osseous canals above described, and is continued by them into the lining membrane of the tympanum."

With regard to the homologies of the above described air-passages,

the author states that the lateral canals answer to the simple Eustachian tubes of Lizards and Mammals, and that the median canal, with its dichotomous divisions, is a speciality peculiar to the Crocodilian reptiles.

The memoir was illustrated by nine drawings of the size of nature.

March 7, 1850.

LIEUT.-COLONEL REID, R.E., Vice-President, in the Chair.

A paper was read, entitled "On the application of Carbon deposited in Gas Retorts as the negative plate in the Nitric Acid Voltaic Battery." By Christopher Leefe Dresser, Esq. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

In the retorts used for the destructive distillation of coal to obtain the carburetted hydrogen gas for the purposes of illumination, after a certain time a deposition of carbonaceous matter takes place, and which at length accumulates to such an extent as to fill up a portion of the retort with solid substance, and to line the whole with a coating varying from the thickness of paper to several inches.

After describing several forms in which this substance occurs, and which vary considerably both in density and hardness, the author states that he found one of great hardness, very little, if at all, porous, and of a stony fracture, to be best adapted for the negative conductor of his nitric acid battery. The most convenient form for the negative conductor is the prismatic, $1\frac{1}{8}$ inch square on the side and about 7 inches long, which is immersed 4 inches in the acid, and used with round porous cells, the zinc cylinder being 3 inches in diameter and $4\frac{1}{2}$ inches high.

The carbon is cut into thin plates or prisms by the machine of the marble cutter, at a cost of about $1\frac{1}{2}d.$ each. The prisms may be easily obtained 12, 14, or 18 inches long.

The only precautions necessary in using this form of carbon, are, after using the plates to immerse them for a few moments in boiling water, to take off the adhering acid, and then to dry them before a fire or in a stove.

Having used the same plates and prisms for months, the author detected no deterioration of their conducting power, nor any decomposition or alteration. The connexion was made by soldering a strip of sheet copper to the zinc, and pressing this strongly against the carbon with a clamp.

Comparing these plates with plates of platinum, the author could detect little difference in action, but the carbon appeared rather superior. He states that his battery of 100 plates cost under £4, whilst one of platinum of equal power would have cost £60 or £70. From the cheapness and durability of this substance, he considers that it will make a valuable addition to our voltaic apparatus.

A paper was also in part read, entitled "Experimental Researches in Electricity." Twenty-third Series. § 29. On the Polar or other condition of Diamagnetic Bodies. By Michael Faraday, Esq., F.R.S. &c.

March 14, 1850.

GEORGE RENNIE, Esq., Vice-President and Treasurer, in the Chair.

The reading of Dr. Faraday's paper, entitled "Experimental Researches in Electricity. Twenty-third Series. § 29. On the Polar or other condition of Diamagnetic Bodies:" was resumed and concluded.

The author, whilst developing, on a former occasion, the phenomena of diamagnetic action, said that all the results might be accounted for by assuming that bismuth, phosphorus, &c., when in the magnetic field, became polar as iron is polar, but with the poles in the contrary direction. This view has since then been adopted by Weber and others, and supported by certain experimental results. In the present paper these results and that view are brought under very close examination. An apparatus was constructed by which a cylinder of any given metal could be moved to and fro through about two inches in the direction of its axis. In doing this it approached close up to, and then retreated from the pole of an electro-magnet, and also moved within a helix of covered wire which was fixed in relation to the magnet. Now the action of such a piece of metal upon the helix is very different in theory, and also in reality, according as it is dependent upon a polarity, magnetic or diamagnetic, acquired by the metal, or upon induced currents existing in the mass; and the question was to ascertain by experiment whether the latter were the cause of the results obtained by Weber and others. The various diamagnetic metals gave the results looked for at the indicating galvanometer; but then these were almost insensible with bismuth, and were greatest with gold, silver, copper, and the better conductors, being indeed in proportion to the conducting power. Such results were in favour of induced currents rather than of polarity. *Division* was next resorted to as a distinguishing test of the polar or current action; thus a cylinder made up of lengths of wires acted as well as a solid cylinder, if the metal were one acquiring a polar state as iron; but such a division interfered with the existence of induced currents in the mass, and it was found that such wire cylinders of copper, &c. lost all power. On the other hand, division of the cylinder into innumerable discs interfered greatly with polarity, but not at all with the induced currents, nor with the action of the diamagnetic metals. The places of maximum and minimum action of a cylinder of metal are very different according as that metal acts by a polar condition, or by currents induced in the mass: it is shown by experiments with the diamagnetic metals that

their places of maximum and minimum action accord with the effects of induced currents. *Time* has great effect over results produced by currents induced in the mass, and none over those due to polarity. By this test the effects of the diamagnetic metals are found due to induced currents.

The phenomena produced by the use of the present apparatus are then shown to be in close and direct relation to the phenomena of revulsion formerly described by the author: the parallel is closely carried out and extended, and both sets of effects referred to one and the same cause.

The author endeavours to repeat an experiment described by Reich, but without success; and he finds that even when iron is used no arrangement of magnets can produce any test of polarity at all comparable to the use of an astatic needle or to suspension between the poles of a powerful magnet, and thinks that arrangements which are thus less sensible with iron are not likely to be more sensible with diamagnetic metals, even if they are polar.

Finally, the author does not consider that the idea of diamagnetic polarity has gained as yet any additional proof beyond the fact that diamagnetic bodies, such as bismuth and phosphorus, are repelled by one or both magnetic poles; he does not reject the idea of polarity, but his opinion or judgment remains the same as at the time of its announcement in 1845.

A paper was also read, entitled "Contributions to the Chemistry of the Urine.—Paper IV. On so-called Chylous Urine." By H. Bence Jones, M.D., A.M., F.R.S. &c.

The definition given of chylous urine is, that it is urine which is white from the suspension of fatty matter in it. An opportunity of observing a case of this disease having occurred to the author, he was led to make the experiments described in this paper. A harness-maker, age 32, half-caste, who had lived in London for twelve years, had been passing such water for nine months. On examination of the water made at 2 P.M. it solidified, looking like blanc-mange in ten minutes. It was very feebly acid, contained fibrin, albumen, blood-globules and fat; specific gravity = 1015. 1000 grs. of this urine gave—

44·42	grs. total solid residue.
8·01	grs. total ash.
14·03	grs. albumen.
8·37	grs. fat.
13·26	grs. urea and extractive matter.
·75	gr. loss.
955·58	grs. water.

In order to watch the variations produced by food and exercise in the appearance of the urine, every time the urine was made, for five days and nights it was passed into bottles marked with the hour. From these observations, and more particularly from the third, fourth, and sixth days, it was evident that the fibrin and albumen appear in the urine when no fat is there, and that the albuminous urine occurs

before food has been taken, and disappears during the night with perfect rest. Thus the fourth day, at 7^h 15^m A.M., on first getting up the urine contained the slightest trace of albumen. The specific gravity=1027; the precipitate by alcohol=0·8 gr. per 1000 grs. urine.

At 9^h 50^m A.M., just *before* breakfast, the urine formed a solid coagulum free from fatty matter, but contained a visible deposit of blood. Specific gravity=1015·6; the precipitate by alcohol=14·1 grs. per 1000 grs. of urine.

At 11 A.M., the urine was chylous or white from fatty matter.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine previous to food are then given.

On five different mornings, by rising early or late, and by collecting the precipitate from the urine by alcohol, the influence of rest and motion was determined. The author states that he could fix beforehand whether the urine should be albuminous or not, by directing the patient to get up, or to lie still.

The patient was bled and the serum was opalescent, but did not clear with æther: the blood contained no excess of fat. 1000 parts of blood gave—

2·63	grs. fibrin.
159·3	grs. blood-globules.
78·1	grs. solids of serum.
240·03	grs. total residue.
759·97	grs. water.

The urine made the same day was examined at different hours; that made immediately before the bleeding was quite white, and that made an hour and a half afterwards was very milky also. Specific gravity=1018. 1000 grs. of urine gave—

56·87	grs. total residue.
10·80	grs. total ash.
13·95	grs. albumen.
7·46	grs. fat.
24·06	grs. urea, &c.
·60	gr. loss.
943·13	grs. water.

The conclusions from these experiments are,—

1. That so-called chylous urine, besides fat, may contain albumen, fibrin, and healthy blood-globules.

2. That, although the fat passes off in the urine after food is taken, yet the albumen, fibrin and blood-globules are thrown out before any food has been taken. During perfect rest the albumen ceases to be excreted; and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early the urine may coagulate spontaneously, although no fat is present; and this may happen previous to food, when the urine is free from fat.

3. Though the urine made just before and a short time after bleeding was as milky as it usually was at that hour of the day, yet

the serum of the blood was not milky: it did not contain a larger quantity of fat than healthy blood does.

The general results are,—

1. That the most important changes in the urine in this disease take place independently of the influence of digestion.

2. That the urine in one respect only resembles chyle, and that is in containing, after digestion, a large quantity of fat in a very fine state of division. The supposition that the disease consists in an accumulation of fat in the blood, which is thrown out by the kidneys, carrying with it albumen, fibrin, blood-globules and salts, is altogether disproved, both by actual analyses of the blood, and by the frequent occurrence of a jelly-like coagulum in the urine when no white fatty matter can be seen to be present.

3. The disease consists in some change in the kidney by which fibrin, albumen, blood-globules and salts are allowed to pass out, whenever the circulation through the kidney is increased; and if at the same time fat is present in the blood, it escapes also into the urine. That this change of structure is not visible to the naked eye on post-mortem examination, Dr. Prout long since demonstrated; and in a case of this disease which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from the urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes, rather than in that of the cortical part of the kidneys.

March 21, 1850.

RICHARD OWEN, Esq., Vice-President, in the Chair.

The following letter from Mr. Addington to the Secretary was read.

Foreign Office, March 20th, 1850.

Sir,—I am directed by Viscount Palmerston to send to you, for the information of the President and Council of the Royal Society, an extract of a letter which his Lordship has received from Mr. James Richardson, stating that in the month of November last, a fall of aërolites had taken place on the coast of Barbary attended with a brilliant stream of light, which extended from Tunis to Tripoli, some of the stones falling in the latter city.

I am, Sir,
Your most obedient, humble Servant,
H. W. Addington.

The Secretary to the Royal Society.

"*Extract of a letter from Mr. Richardson, dated off Jerbah, 25th January* 1850.

"I will trouble your Lordship by the mention of the astronomic

phenomenon which terrified or arrested the attention of the inhabitants of the whole of this coast some two months ago. This was the fall of a shower of aërolites, with a brilliant stream of light accompanying them, and which extended from Tunis to Tripoli, some of the stones falling in the latter city.

"The alarm was very great in Tunis, and several Jews and Moors instinctively fled to the British Consulate, as the common refuge from every kind of evil and danger.

"The fall of these aërolites was followed by the severest or coldest winter which the inhabitants of Tunis and Tripoli have experienced for many years."

The reading of a paper, entitled "Discussion of Meteorological Observations taken in India at various heights." By Lieut.-Colonel Sykes, F.R.S. &c., was commenced, but was not concluded.

The Society then adjourned over the Easter recess, to meet again on the 11th of April.

April 11, 1850.

PROFESSOR OWEN, Esq., Vice-President, in the Chair.

Lieut.-Colonel Sykes's paper, entitled "Discussion of Meteorological Observations in India," was resumed and concluded.

The author adverts to a former paper "On the Meteorology of the Deccan," published in the Philosophical Transactions for 1835, and after referring to the conclusions at which he arrived in that communication, states that, in the discussion of the meteorological observations which form the subject of the present paper, and which were made over a very extended area, at different heights, some being hourly and running through several years at the same station, it is very satisfactory to find that they fully establish the accuracy of the former deductions. He remarks that, as some of the observations now discussed were hourly records continued through considerable periods of time, an opportunity has been afforded of investigating abnormal conditions, which the former limited number of diurnal observations did not permit; and gives the following review of what appears to be normal and abnormal conditions.

The annual and daily range of the barometer diminishes from the sea-level up to the greatest height observed, 8640 feet at Dodabetta, from a mean annual and mean daily range at Madras of 0·735 and 0·122 respectively to 0·410 and 0·060 at Dodabetta;—the annual range would appear to increase, about and beyond the northern tropic, as the annual range at Calcutta (not by hourly observations) is 0·911; but the diurnal range is somewhat less (0·115) than at Madras. At no one of the places of observation, even taking the maximum pressure of one year with the minimum pressure of another year, does there appear to have been a range of pressure equivalent

to an inch of mercury; nevertheless in the Cyclones, or rotatory storms, there occurs at times a range of pressure of nearly two inches of mercury within forty-eight hours; but it is shown from a comparison of the simultaneous records on board ship, where these great depressions were noted, with the records at the observatories on shore, that the great depressions occurred within very limited areas.

The author had formerly shown that the times or turning-points of ebb and flow (if the term be permitted) of the aërial ocean, were occasionally retarded or accelerated, although the means fixed the turning-points within certain limit hours; but he was not then aware that in the ebb or flow of the four daily tides, they ever retrograded or halted in their onward or retiring course. The hourly observations now show that abnormal conditions are of no infrequent occurrence,—that the tides at times flow or ebb for four, five, six or even seven and eight hours (one instance at Aden of nine hours),—that frequent instances occur of retrograde movements for short periods of time, as if the tide had met with a check and been turned back; and at the turning-points there are numerous instances of the atmosphere being stationary for a couple of hours.

The maximum pressure of the atmosphere is in the coldest months, December or January, but the minimum pressure is not in the hottest months, but in June or July. The barometric readings, when protracted, show a gradual curve from December or January descending to June or July, and then ascending again to December or January, there being an occasional interruption in October or November. As the curves at Madras, Bombay and Calcutta, correspond, and as Madras has *no* south-west monsoon, while Bombay has a south-west monsoon, and is destitute of the north-east monsoon of Madras, it would appear that the general movements of the atmosphere are little influenced by any conditions of its lower strata; but the curve of pressure would seem to have some relation to the sun's place in the ecliptic.

The normal conditions of daily temperature are, that it is coldest in India at sunrise, and hottest between the hours of 1 and 3 P.M.; but the tables show many aberrations from this rule. The regular increment or decrement of mean monthly heat from the maximum or minimum period is somewhat remarkable, as the curve is independent of the south-west monsoon at Bombay and the north-east monsoon at Madras; and the passage of the sun twice over both places does not derange the curve. The anomalies of the annual mean temperature of Madras, Bombay, Calcutta and Aden, not diminishing with the increase in the latitude of the respective places, are pointed out, and numerous instances are given of the very great power of the slanting rays of the sun beyond the tropic. As is the case with the barometric, so do the heat tables indicate that the annual and daily ranges of the thermometer diminish with the elevation of the place of observation above the sea-level, the elevated table-land of the Deccan however being an exception to this rule. At Mahabuleshwur, at 4500 feet, the temperature of the air was never below 45° with a maximum and minimum thermo-

meter; and at Dodabetta the temperature of the air was never below 38°·5, nevertheless at both places ice and hoar-frost were frequently found on the ground at sunrise, resulting from the separate or conjoined effects of radiation and evaporation.

After stating the want of confidence he has in observations of the wet-bulb thermometer as a means of determining the dew-point, and that he greatly prefers Daniell's hygrometer for this purpose, the author observes that he will not venture to say more with respect to normal conditions of moisture in India than that the air of the sea coast has always a much greater fraction of saturation than the lands of the interior; and that the elevated plateau of the Deccan is periodically subject to very high degrees of dryness.

Some very unexpected phenomena with reference to the distribution of rain are pointed out. It is found both on the sea coasts and on the table-lands of the Deccan, that within very limited areas, the differences in the fall of rain may be very great. With nine rain-gauges employed in the small island of Bombay in the months of June and July, in the monsoon of 1849, the quantity collected in the different gauges ranged in July from 46 inches to 102 inches, and in June from 19 inches to 46 inches. At Sattarah, in the Deccan, with three rain-gauges within the distance of a mile, they differed in their contents several inches from each other; and at Mahabuleshwur and Paunchgunny, nearly on the same level, the latter place being only eleven miles to the eastward of the former, the annual fall of rain was 254 inches and 50 inches respectively! The normal conditions are, that there is a much greater fall of rain on the sea coasts than on the table-lands of the Deccan, but that the Ghats intervening between the coasts and the table-lands have three times the amount of the fall on the coasts, and from ten to fifteen times the amount of the fall on the table-lands of the interior; the paucity of the fall of rain at Cape Comorin and in the mouths of the Indus would also appear to be normal conditions.

The tables must be referred to for the winds; the normal states are those of the south-west and north-east monsoons, and the influence of the latter is periodically felt at the height of 8640 feet at Dodabetta, which height would appear just at the upper surface of the stratum of air constituting the south-west monsoon; but hourly observations for lengthened periods are necessary at Dodabetta, to determine what really are the periodical winds at that height. From the points other than those between south and west, and north and east, there is also at the several stations a certain amount of periodicity in the winds, the winds that are common to different stations having only a slant more or less at the different stations; for instance, the south-west and north-west winds of Bombay blowing in the summer months in Calcutta incline rather to be south and north winds, than south-west and north-west winds; but the author observes that to be enabled to speak with any precision upon this branch of the meteorology of India, and indeed upon most other branches with a comprehensive and philosophical object, hourly observations are necessary,—simultaneously taken with previously compared instruments by zealous observers;

and having the records in a form common to all the observers, so as to admit of rigid comparisons:—when this is done, not only in India but in Europe, meteorologists will be in a better condition to generalize and propound normal conditions, than the state of our knowledge at present would justify.

The author states that he is indebted to that very able and zealous meteorologist, Dr. Buist of Bombay, for the protracted curves of pressure of the barometer appended to his paper.

A paper was also read, entitled "On the Structure and Use of the Ligamentum rotundum Uteri, with some observations upon the change which takes place in the structure of the Uterus during Utero-gestation." By G. Rainey, Esq., M.R.C.S.E., Demonstrator of Anatomy, St. Thomas's Hospital. Communicated by Joseph H. Green, Esq., F.R.S.

The author first refers to the discovery of the difference which exists between the two classes of muscles; the voluntary, or those with striped fibres, and the involuntary, or those with unstriped fibres. He then notices that the opinion which is entertained respecting the round ligaments being composed of the unstriped variety of muscular fibre is incorrect, these organs consisting chiefly of the striped muscular fibre

In support of the accuracy of this assertion, the author alleges the following facts:—

First, that the round ligament arises by three tendinous and fleshy fasciculi; one, from the tendon of the internal oblique, near the symphysis pubis, a middle one from the superior column of the external abdominal ring, the third from the inferior column of the same: from these points the fibres pass backwards and outwards, and uniting form a rounded cord—the round ligament; after which, traversing the broad ligament, they go to be inserted into the angle of the uterus.

The striped fibres are principally situated in its centre, and extend from its origin to within an inch or two of the fundus uteri; as they approach which, the fibres gradually lose the striated character and degenerate into fasciculi of granular fibres of the same kind as those of the Dartos muscle; both these fibres presenting similar microscopic characters when acted upon by glycerine.

The author then states that the round ligament does not pass through the external ring to be lost in the labia and mons veneris; and argues from the fact of their consisting mainly of striped fibres, &c., that their use cannot be merely mechanical or subservient to the process of utero-gestation, and therefore he concludes that its function must be connected in some way with the process of copulation.

He also adverts to the necessity of examining the round ligament by the microscope in glycerine in preference to any other fluid; as this substance renders the cellular tissue mixed with the fibres more transparent without diminishing the distinctness of their characteristic markings. The author next states his views on the changes

which take place in the uterus during utero-gestation, and observes, first, that there is no similarity between the fibres of the round ligament and those of the unimpregnated uterus, the latter being made up of spindle-shaped nucleated fibres, contained in a matrix of exceedingly coherent granular matter; that these fibres are best examined in portions which have been broken up by needles, in preference to thin sections; and that this tissue is well seen in the larger mammals, as in the Cow, &c. In the impregnated uterus the fibres are found much increased in size and distinctness, but devoid of nuclei and comparatively loosely connected; and the enlargement of these fibres is of itself sufficient to account for the increased volume of the gravid uterus, without supposing that a set of muscular fibres are formed in it *de novo*.

Hence he reasons that the unimpregnated uterus consists probably of little more than an assemblage of embryonic nucleated fibres, inactive until the ovum is received into it, after which their development commences and continues simultaneously and progressively with that of the fœtus; so that when this last has arrived at a state requiring to be expelled, the uterus has acquired its greatest expulsory power. Lastly, the author observes, since the fully-developed fibres cannot return to their former embryonic condition, they necessarily become absorbed, and a new set of embryonic fibres are formed for the next ovum, so that each fœtus is furnished with its own set of expulsory fibres; which view is in perfect accordance with the statements of Drs. Sharpey and Weber, with regard to the membrana decidua.

April 18, 1850.

The EARL OF ROSSE, President, in the Chair.

Robert Stephenson, Esq., was admitted into the Society.

The following papers were read:—

1. "On the Solution of Linear Differential Equations." By the Rev. Brice Bronwin, M.A. Communicated by S. Hunter Christie, Esq., Sec. R.S.

The methods employed in this paper to effect the solution or reduction of linear differential equations consist of certain peculiar transformations, and each particular class of equations is transformed by a distinct process peculiarly its own. The reduction is effected by means of certain general theorems in the calculus of operations.

The terms which form the first member of the first class of equations are functions of the symbols ϖ and τ, the latter being a function of x, and the former a function of x and D, x being the independent variable. This member of the equations contains two arbitrary functions of ϖ, and may therefore be of any order whatever. It likewise contains two simple factors, such for example as ϖ and $\varpi+nk$, which factors are taken away by the transformation em-

ployed, and consequently the equation is reduced an order lower; it is therefore integrated when of the second order. There is a series of equations of this class, each essentially distinct from the rest, yet all reducible by a similar process.

These equations contain two arbitrary functions of x. The number therefore of particular practicable forms, which may be deduced from each, is very great, a circumstance which renders our chance of putting any proposed equation under one of these forms greater in the same proportion. On account of the very large number of particular integrable equations which each general example furnishes, selection would be very difficult, and all could not be given; the author has therefore refrained from giving any.

The second class of equations may be deduced from the first by the interchange of the symbols D and x, and changing τ into τ^{-1}. The second general theorem can be deduced from the first in like manner; and this class may be transformed and reduced by it in a manner exactly similar to that by which the former class is reduced by the first general theorem. The solution therefore of the one series may be deduced from that of the other by the interchange of symbols only. But in the second series the solutions obtained are not always practicable, that is to say, they cannot always be interpreted in finite terms. They have therefore been reduced by the introduction of new arbitrary functions of D, which render them practicable; this process however necessarily diminishes their generality.

When reduced to the ordinary form, these equations are somewhat complicated; but by giving suitable forms to the arbitrary functions of D which they contain, we may derive from them particular examples of a form as simple as we please, and by introducing as many arbitrary constants as possible, these examples may be made very general of the class to which they belong. In the integration of linear equations, the coefficients of which are integer functions of x, they may prove very useful.

Next, an equation, a particular case of which was treated by Mr. Boole in the Cambridge Mathematical Journal, is here integrated under its most general form. Instead of integer functions of x, the coefficients may be any functions whatever, consistent with the condition of integrability, which is ascertained, and the formulæ of reduction assumed by Mr. Boole are shown to be universally true. An additional function of the independent variable is also introduced into the operating symbol π. The equation therefore, independently of the condition of integrability, contains two arbitrary functions of x, and consequently gives rise to a considerable number of particular integrable examples.

Here also the interchange of the symbols D and x is made, both in the equation to be integrated and in the general symbolical theorem by which it is reduced, and the same reduction to practicable forms as before is likewise made.

The next class of equations results from the generalization of another equation integrated by Mr. Boole in the Cambridge Mathema-

tical Journal. Here the symbol D of Mr. Boole is replaced by the general symbol ϖ, and moreover the first member of each equation contains two arbitrary functions of ϖ; and by means of another extension, this example gives rise to a whole series of equations constituting a class. The reduction is effected partly by the first general theorem in the calculus of operations, and partly by other means. It must be observed that each of the classes is totally distinct from the others, and its mode of treatment also distinct; also each of the general examples in the series contains two arbitrary functions of the independent variable, and will therefore give the solutions of a large number of particular equations, but for the reason before stated particular examples are not given.

Here likewise, by the interchange of the symbols D and x, another series of equations with their solutions or reductions is obtained, and also another general theorem by which they may be transformed and reduced. But the solutions of the examples of the one series may be deduced from those of the other by the interchange of symbols. It is not a little remarkable that this interchange of symbols in all these cases should be found possible, it will however be found possible in another case to be hereafter described.

The last class of equations discussed in this paper is transformed by means of a general theorem of a very different kind from any of those which have been employed in reducing and integrating any of the previous classes. By means of this transformation, the symbol ϖ, of which the first member of these equations is a function, is placed in a position to operate upon the whole of that member, a certain equation of condition among the coefficients being previously admitted. Hence by operating upon both members with the inverse of this symbol, the equation is once integrated, and, if it be of the second order only, completely solved.

Here too the interchange of symbols may be made both in the equation and its solution, and the solution so changed will be the solution of the equation changed in like manner. The general symbolical theorems, which here consist of a series of terms, may be derived the one from the other in the same way, and by changing the signs of the alternate terms.

Reductions of the arbitrary functions of D, similar to those before made, are made here also; and by particularizing some of the functions so reduced for the sake of simplification, several very singular resulting equations are obtained. If in these we assign to the remaining arbitrary functions, particular forms, and introduce as many arbitrary constants as we can, we may find particular examples which may be of great use in the integration of equations with coefficients containing only integer functions of x.

By a very obvious substitution an arbitrary function of x may be introduced into any of this kind of equations, and also another function of D, and the last often with great advantage.

2. "On the Oils produced by the action of Sulphuric Acid upon various classes of Vegetables." By John Stenhouse, Esq., F.R.S.

Nearly thirty years ago Döbereiner observed, when preparing formic acid by distilling a mixture of starch, peroxide of manganese and sulphuric acid, that the liquid which passed into the receiver contained a small quantity of oil which rendered it turbid. To this oil Döbereiner gave the fanciful name of "artificial oil of ants," though the very limited quantity in which he was able to procure it prevented him from determining almost any of its properties.

The author's attention was first directed to the subject in 1840, when he found that the oxide of manganese was unnecessary, and that the oil could be readily prepared by operating on most vegetable substances with either sulphuric or muriatic acid. The oil, on analysis, was found to have the formula $C_{15}\ H_6\ O_6$, and to contain oxygen and hydrogen in the proportions to form water, while all other oils and fats contain an excess of hydrogen.

The late Dr. Fownes took up the subject in 1845, and made the interesting discovery, that when the oil which he called furfurol is heated with ammonia, a crystalline amide is formed. When this amide is boiled with caustic lyes, it is changed into the crystallizable base furfurine. The paper then describes the best mode of preparing furfurol, and also the method of purifying it from an oil with which crude furfurol is always accompanied, and to which the name of meta-furfurol has been given. Meta-furfurol is the cause of the bright red coloration which impure furfurol instantly produces when it is treated with muriatic or sulphuric acids in the cold. This portion of the paper concludes with some new observations on furfurol, and an examination of some of the salts of furfurine.

It has been pretty satisfactorily ascertained that the constituent of plants which yields furfurol is the *matière incrustante* of M. Payen, viz. the matter with which the interior of the cells of plants is lined. This is an amorphous granular substance which has been gradually deposited from the sap in its passage through the tissue of the plant. It is most abundant in hard woods, such as oak and teak, especially in the oldest portions which lie nearest the heart of the tree. As the author of the paper was led to conjecture that the *matière incrustante* of the different great classes of vegetables would be found on examination analogous but not identical, he thought it likely that the oils derivable from them would also prove not identical with furfurol, though probably very analogous to it in their nature and properties. The algæ or sea-weeds, whose structure is very different from that of ordinary herbaceous plants, were employed to test the truth of this hypothesis. They yielded an aromatic oil to which the name of fucusol was given. Though essentially different from furfurol, it closely resembles that oil in its properties, being also isomeric with it. Fucusol forms a crystallizable amide with ammonia, called fucus amide, which, when it is boiled with alkaline lyes, is also converted into an organic base—fucusine, which is likewise isomeric with furfurine. Fucusine is a rather difficultly crystallizable base; but some of its salts, especially the nitrate, may be readily procured in large crystals. In solubility and crystalline form they differ from those of the corresponding base.

The paper contains an analysis of these salts.

The mosses and lichens were also found to yield fucusol. The ferns, on the other hand, yield a peculiar oil, which differs both from fucusol and furfurol, possessing properties intermediate between them.

The results of these experiments seem to indicate some curious botanical relations, as it appears highly probable that the *matière incrustante* is the same in all phanerogamous plants as they yield furfurol. On the other hand, the *matière incrustante* in the Algæ, mosses and lichens, as it yields fucusol and not furfurol, though the same in each of these classes, is evidently different from that of phanerogamous plants. The *matière incrustante* of ferns appears however to be dissimilar from either of the others, as it yields an analogous but peculiar oil.

April 25, 1850.

The EARL OF ROSSE, President, in the Chair.

M. Quetelet was admitted into the Society.

The following papers were read:—

1. "On the Temperature of Steam and its corresponding Pressure." By John Curr, Esq. Communicated by J. Scott Russell, Esq., F.R.S.

The author states that it is intended in this paper to propose a simple law to determine the pressure of steam corresponding to any given temperature, *irrespectively of experiment*, taking as the sole datum, that the vaporizing point of water under a given pressure is 100 degrees, that number being taken to correspond with the scale of Celsius; also to construct formulæ in accordance therewith; and afterwards to compare their results with the actual experiments of the Academy of Sciences of Paris. He further states that the rationale of the subsequent formulæ is expressed as follows. Let it be conceived that a *given* quantity of water is vaporized under the condition that the pressure thereon is increased in the same ratio that the volume is increased, or that at intervals of temperature 1, 2, 3, &c. the volume is increased the same or in equal proportions; the temperature of the volume will be increased exactly as the square of the temperature indicated by the thermometer, supposing the instrument to be a true measure of temperature, and as the square of the volume; and the same of the pressure.

Steam being generated from an *indefinite* quantity of water and confined within a limited space, as in the usual boiler, he considers the foregoing case is reversed; for the volume being constant, the action of the fire is entirely exerted in producing increased elastic tension of the vapour; therefore the temperature of the steam at the interval 1 to 2 degrees is increased inversely in the duplicate ratio of the ratio in the case first described; that is, the pressure is increased directly at the square of the square, or fourth power of the

temperature; whence the following law. The pressure of steam generated in the usual steam-boiler is directly proportional to the fourth power of its temperature, when measured by a true scale.

It being assumed that 100 degrees is the temperature of steam when its pressure is in equilibrium with a column of 30 inches of mercury, or with the pressure of one atmosphere, then F being the pressure in atmospheres, at any temperature t,

$$F=\left(\frac{t}{100}\right)^4.$$

A comparison is instituted between theoretic experiments of the Academy of Sciences and the results of this formula, from which it appears that the temperatures deduced from the formula are invariably in defect, the greatest difference being 3·51, and the least 2·02.

2. "On the means adopted in the British Colonial Magnetic Observatories for determining the absolute values, secular change, and annual variation of the Magnetic Force." By Lieut.-Col. Edward Sabine, R.A., For. Sec. R.S.

The determination of the mean numerical values of the elements of terrestrial magnetism in direction and force at different points of the earth's surface (the force being expressed in absolute measure, intelligible consequently to future generations, however distant, and conveying to them a knowledge of the present magnetic state of the globe), and the determination of the nature and amount of the secular changes which the elements are at present undergoing, are, as the author states, the first steps in that great inductive inquiry by which it may be hoped that the inhabitants of the globe may at some date, perhaps not very distant, obtain a complete knowledge of the laws of the phenomena of terrestrial magnetism, and possibly gain an insight into the physical causes of one of the most remarkable forces by which our planet is affected.

After stating the inadequacy of the instruments originally proposed by the Royal Society, to the attainment of all the objects for which they had been designed, the author refers to the modifications which had been introduced, in the instruments and methods of observation for the determination of the absolute values, and the secular changes of the horizontal component of the magnetic force. He then gives the series of the results of the monthly observations at Toronto from January 1845 to April 1849 as relatively correct; and from this series, regarding each monthly determination as entitled to equal weight, and taking the arithmetical mean of all the values as the most probable mean value, obtains 3·53043 as the mean value of the horizontal force at Toronto, with a probable error of $\pm$·00055; and the probable error of $\pm$·0040 for each monthly determination.

This is on the most simple hypothesis, in which neither secular change nor annual variation is supposed to exist. The monthly results however distinctly indicate a secular change, and by means of them, on the hypothesis of a uniform secular change, the author

deduces ·0042 as the annual decrease of the horizontal force during the period comprehended by the observations, the value of the force on the 1st of March 1847, the mean epoch being 3·53043, with a probable error of $\pm$·00025.

For the purpose of deducing the values of the total magnetic force and its secular change from those of the horizontal force, it is necessary to know the magnetic inclination corresponding to the epoch and its secular change. From the observations of the inclination, 75° 16′·09 is deduced as the value of this element on the 1st of March 1847, with a secular increase of 0′·89 annually; and 13·8832 as the value of the total force in absolute measure, at the same epoch. As the annual increase of 0′·89 in the inclination will not account for an annual decrease of more than ·0033 in the horizontal force, there remains ·0009 as indicative of a small annual decrease in the total force during the period of the observations, and the author considers that the probabilities are in favour of such a decrease.

The general fact of an annual variation of the horizontal force at Toronto, the force being greater in the summer than in the winter months, is shown by three independent methods of experiment, viz. the observations from which the foregoing conclusions have been drawn, the regular observations with the bifilar magnetometer, and observations undertaken expressly with the view of ascertaining the fact. The author also infers the probable existence of an annual variation of the total force, the force being greatest in the winter months, or when the sun is in the southern signs, and least in the summer months, or when the sun is in the northern signs.

The results obtained from the observations at Hobarton are next briefly stated. The investigation, conducted in the same manner as at Toronto, shows at Hobarton a decrease of *south* inclination of 0′·89 on the average of the months from April to August inclusive, that is, in the southern winter; and an increase of 0′·85 from October to February inclusive, that is, in the southern summer.

The series of observations on the horizontal force shows an annual variation of the same character as respects the seasons, and almost identical in amount with that at Toronto. In the months from October to February inclusive, or in the summer months at Hobarton, the horizontal force is ·0017 greater on the average than its mean amount; and from April to August inclusive, or in the winter months at Hobarton, it is on the average ·0013 less than its mean amount.

The inferences drawn from these variations of the inclination and horizontal force, taken jointly as respects the total force at Hobarton, are that this force is subject to an annual variation, being higher than its mean amount from October to February, and lower than its mean amount from April to August.

It thus appears that in the months from October to February the magnetic needle more nearly approaches the vertical position, both at Toronto in the northern hemisphere, and at Hobarton in the southern; and that the total force is greatest at both stations from October to February, and least from April to August.

It is much to be desired, the author states, that so remarkable a result should receive a full confirmation, by the continuance of the observations at Toronto and Hobarton for such an additional period as may appear necessary for that purpose; and that the general conclusion, indicated by the observations at those stations, should be verified by similar investigations in other parts of the globe, especially at the observatories which now exist. He conceives that these facts indicate the existence of a general affection of the whole globe having an annual period, and would appear to conduct us naturally to the position of the earth in its orbit as the first step towards an explanation of the periodic change. He further urges the importance of following up without delay, and in the most effective manner, a branch of the research which gives so fair a promise of establishing, upon the basis of competent experiment, a conclusion of so much theoretical moment.

In conclusion the author adverts briefly to considerations which may give a particular importance to accurate numerical values of the magnetic elements and their secular changes at Toronto, namely the proximity of that station to one of the two points on the northern hemisphere, which are the centres of the isodynamic loops, and are the points of the greatest intensity of the force (on the surface of the globe) of apparently two magnetic systems, distinguished from each other by the very remarkable difference in the rate of secular change to which the phenomena in each system appear to be subject.